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Essays on the Economics of State Policy Reform

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Essays on the Economics of State Policy Reform

by

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Essays on the Economics of State Policy Reform

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Lately, the debate over various public policies, such as immigration reform and tax policy, has heated up in the United States. This dissertation seeks to explore the different impacts that some of the policy changes have on different groups of people. In doing so, I am able to help better inform policymakers of the possible economic outcomes of future reforms.

The first chapter examines the labor market impacts of two state-level immigration policies designed to reduce the presence of undocumented immigrants: E-Verify and “Show Me Your Papers” (SMYP). Using a difference-in-difference strategy, I examine the separate and combined effects of these laws on the employment and wages of likely unauthorized, working-age men and women and the groups of low-skill American workers with whom they are most likely to compete for jobs. I also look at how these laws impact state-level economic outcomes, including industry-specific GDP. I find that immigration reform reduces employment and hourly wages among undocumented men. Immigration reform also results in large, negative impacts on state GDP, especially in industries that rely more heavily on undocumented workers.

The second chapter examines the questions of whether consumers respond differently to taxes of different salience levels and if there is heterogeneity in consumer tax salience across

income groups and other categorical groups such as age and education groups. I find evidence supporting tax salience theory in the market for alcohol. Additionally, I find evidence of heterogeneity in tax salience effects across different education levels. In particular, more educated consumers are more responsive to changes in sales taxes.

The third chapter focuses on the impacts of immigration reform on the children of undocumented immigrants. By comparing siblings in a difference-in-difference approach, I show that DACA, a policy that reduces legal barriers for young undocumented immigrants, increases the educational attainment of potentially eligible youth. Meanwhile, policies such as Alabama HB 56, which increase barriers for undocumented immigrants, reduce the enrollment rates and increase dropout rates for the children of undocumented immigrants.

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Chapter 1

Sweet Casa Alabama (and Arizona, and...): Examining the Economic Outcomes of State Immigration Reform

1.1 Introduction

During the the 2016 United States Presidential Election, the topic of unauthorized immigration became increasingly debated nationwide with eventual President Donald Trump going on record supporting policies such as building a border wall between the US and Mexico and cracking down on sanctuary cities. Since taking office President Trump has taken other steps designed to remove undocumented immigrants from the US, such as ending the Deferred Action for Childhood Arrivals (DACA) program. Though he often argues that the rationale behind such policies is to reduce crime, President Trump has also framed his stance in labor market terms. For instance, at a 2016 campaign rally in Arizona, he said, "While there are many illegal immigrants in our country who are good people, ...this doesn't change the fact that most illegal immigrants are lower skilled workers with less education, who compete directly against vulnerable American workers and... draw much more out from the system than they can ever possibly pay back."¹

President Trump is far from the only American to feel this way about undocumented immigrants. According to a series of 2017 polls by Gallup, 40% of Americans feel that immigrants

¹Source: New York Times <https://www.nytimes.com/2016/09/02/us/politics/transcript-trump-immigration-speech.html>

mostly hurt the economy, and 39% of Americans worry about undocumented immigration a “great deal.”² With these current attitudes and the political climate, it is now of the utmost importance to understand the true impact that unauthorized immigrants have on the US economy.

This paper sheds light on this issue by examining the impact that state laws targeting unauthorized immigrants have on labor markets and state economies. Do tough immigration policies lead to a reduction in employment and wages for unauthorized immigrants? Do US citizens experience a corresponding bump in employment and wages? Are undocumented immigrants a net positive or negative for economic productivity? To answer these questions and examine the mechanisms behind the observed changes, I utilize cross-state variation in different types of immigration reforms. I focus specifically on two policies: E-Verify mandates and “Show Me Your Papers” (SMYP) laws.

Since 2006, several states have implemented policies designed to target undocumented immigrants. Perhaps the most publicized of these policies was Arizona SB 1070, an omnibus immigration enforcement law that gained notoriety in 2010 due to its SMYP provision allowing police to inquire about the immigration status of anyone they have reasonable suspicion might be in the country illegally during the course of any legal stop. A handful of other states implemented laws containing similar SMYP provisions. Many more states mandated the use of E-Verify by employers. E-Verify is a system that verifies whether or not a potential hire is legally authorized to work in the US.³ Both SMYP laws and E-Verify mandates attempt to identify unauthorized immigrants and remove them from local labor markets, though they operate through different mechanisms. E-Verify laws place incentives on employers to not hire unauthorized workers and incentives on

²Source: Gallup <http://news.gallup.com/poll/1660/immigration.aspx>

³For more detailed descriptions of E-Verify mandates and SMYP laws, see Section 2.

unauthorized workers to not apply for jobs in the state. In theory, this could reduce both the labor demand for and labor supply of undocumented workers. SMYP laws affect the labor market via reducing the number of unauthorized immigrants in the state due to (the threat of) deportations. This would reduce the supply of undocumented workers and could reduce labor demand as well if employers are concerned that undocumented workers have a higher chance of not being able to work.

While these laws apply to all unauthorized immigrants, regardless of background, the effect on unauthorized Hispanics is likely to be particularly heavy. According to a 2014 report by the Pew Research Center, there were roughly 11.2 million unauthorized immigrants in the United States in 2012.⁴ Of these immigrants, 52.4% were born in Mexico, 15.2% in Central America, 6.3% in South America, and 4.9% in the Caribbean. Additionally, nearly 10.4 million of these immigrants are adults. In 2012, California had by far the largest population of unauthorized immigrants with nearly 2.5 million. Texas had 1.7 million, while Florida, New York, New Jersey, Illinois, Georgia, North Carolina, and Arizona all had populations of unauthorized immigrants of at least 400,000. From 1990 to 2007, the population of unauthorized immigrants rose rapidly and steadily to a peak of around 12.2 million. In part due to the Great Recession, this population fell before leveling off at its 2012 levels. While the actual number of unauthorized immigrants and growth rate have fallen from their peak levels, the median length of residence increased from 8.6 years in 2007 to 12.7 years in 2013.

In addition to having an impact on unauthorized workers, immigration reform could also

⁴This number is estimated using American Community Survey (ACS) IPUMS 1% samples by focusing on the Hispanic immigrants who are most likely to be unauthorized. I use a similar methodology to identify unauthorized immigrants, described in detail in Section 4.

have an effect on the labor market behaviors and outcomes of legal immigrants and US citizens. If the common belief that immigrants are occupying jobs that would otherwise be held by Americans is true, then any policies that make the labor market less favorable for unauthorized immigrants should improve outcomes for everyone else with similar skills. To this end, I also examine the impact of SMYP and E-Verify mandates on the groups of people most likely to be competing with unauthorized immigrants for jobs. This will help determine whether US-born or naturalized citizens are strong labor substitutes with unauthorized immigrants.

Previous literature on the impact of recent state immigration reform has typically focused on either E-Verify mandates or SMYP laws but never both simultaneously. There is not much consensus in the literature on whether E-Verify mandates reduce the employment or wages of undocumented workers. Amuedo-Dorantes and Bansak (2012) find that unauthorized men and women experience large decreases in employment in states that mandate E-Verify but no wage decreases. In fact, they find that unauthorized women's wages actually increase. However, Orrenius and Zavodny (2015) find that E-Verify mandates reduce the wages of unauthorized men, but find no evidence that employment changes. More recently, Borjas (2017) estimates that E-Verify mandates reduce the wages of undocumented immigrants by about 2%. There is also a lack of consensus on the effects that E-Verify mandates have on US citizens. Both Amuedo-Dorantes and Bansak (2012) and Orrenius and Zavodny (2015) find generally positive labor market outcomes for naturalized Hispanic citizens, US-born Hispanics, and non-Hispanic whites. On the other hand, Bohn, Lofstrom, and Raphael (2015) find that E-Verify actually reduced the employment of non-Hispanic citizens in Arizona. Finally, Orrenius and Zavodny (2016) show that states that mandate E-Verify experience reductions in unauthorized immigrant populations. All of these studies and many others like them ignore the potential impact that other types of immigration laws, such as

SMYP, might have on their results.

SMYP immigration reforms remain less studied. Existing literature has tended to focus on migration and border crossing outcomes. Amuedo-Dorantes and Pozo (2014) find that these types of laws reduce the number of deportees that attempt to reenter the United States. Hoekstra and Orozco-Aleman (2017) find that Arizona SB 1070 deterred entries into the state from Mexico by at least 30%. Sanchez (2015) finds that Arizona SB 1070 temporarily reduced the proportion of noncitizen Hispanics in the state. Good (2013) finds that immigration reform leads to decreases in both the total population and employment for likely unauthorized immigrants. Lastly, Zhang, Palma, and Xu (2016) find that Alabama HB 56 led to an increase in violent crime in the state.

This paper builds off of and contributes to the existing literature in several key ways. This is the first paper to explicitly consider the impact that SMYP laws can have on labor markets. This is important for a couple key reasons. First, many proposed immigration policies would likely be enforced by police or immigration officers rather than solely by employers. Thus, my findings can be useful to better understanding the possible impacts of future immigration legislation. Second, I show that SMYP has effects on employment and wages beyond E-Verify mandates alone. By including both SMYP and E-Verify mandates, I can examine the possibility that the two laws have complementary effects when implemented together and test whether or not previous studies suffer from omitted variable bias since many of the states with the strongest E-Verify laws also implement SMYP laws. This is also the first study to examine the impact of these immigration reforms on state macroeconomic outcomes, such as GDP. In doing so, I show that SMYP and E-Verify have large economic effects beyond the individual labor market decisions of undocumented immigrants and the US-citizens competing with them. Other contributions include providing extensive analysis designed to uncover the underlying mechanisms behind the observed impacts of immigration

reform and using datasets, such as the TRAC Immigration Customs and Enforcement data, that have previously been underutilized in this literature.

Using a difference-in-difference framework, I find that both SMYP and E-Verify mandates reduce employment by up to 3 percentage points and wages by 2–3% for likely unauthorized Hispanic men. These effects are larger in states that implement both types of immigration reforms. I show that these changes are likely the result of decreased labor supply and decreased labor demand for undocumented workers and provide evidence that SMYP operates through a different channel (law enforcement) than E-Verify mandates. While undocumented men have adverse labor market outcomes, undocumented women remain largely unaffected. Surprisingly, US citizens remain mostly unaffected as well. There is little evidence that their employment increases following immigration reform. Finally, I find that state GDP per capita decreases significantly following immigration reform by up to 3%. These decreases are most heavily concentrated in industries that traditionally rely on undocumented labor such as agriculture and construction.

The outline of this paper is as follows. Section 2 provides background on E-Verify mandates and SMYP laws in the United States. Section 3 describes a simple economic framework for thinking about how these policies can impact employment and wages. Section 4 provides a description of the data used in this analysis. Section 5 describes the empirical model I use for estimation. Section 6 discusses the results of this analysis. Section 7 concludes.

1.2 Background on Laws

Modern efforts to create legislation designed to remove unauthorized immigrants from the US economy began in 1986 with the passage of the Immigration Reform and Control Act (IRCA). Among other provisions, the IRCA made it illegal for employers to knowingly hire undocumented

workers. The IRCA also provided amnesty and citizenship for any illegal immigrants residing continuously in the United States since 1982 with no criminal record. Mandated employer usage of Form I-9, the Employment Eligibility Verification, for new hires also has its roots in the IRCA.⁵

1.2.1 E-Verify

E-Verify can be thought of as an extension of the IRCA's Form I-9 requirements. E-Verify is an internet-based program that allows employers to verify employment eligibility at no cost. It is run by the Department of Homeland Security (DHS) in conjunction with the Social Security Administration (SSA). Participating employers enter a new employee's Form I-9 information into the E-Verify system within three business days of that employee's first day of paid work. This information is then compared to millions of government records to determine whether or not an employee is eligible to work.⁶ This process typically lasts under 24 hours. If E-Verify determines that there may be an eligibility issue, the employee has 8 business days to contact the SSA or DHS to resolve the problem. During this time, the employee is legally allowed to continue working for the firm. E-Verify is stricter than the Form I-9 requirement as it requires an employee to enter a social security number and provide photo identification.

E-Verify first started in 1997 as a government pilot program. From its inception, E-Verify had been available for employers to use voluntarily. However, starting with Colorado in 2006, some states have mandated that all public employers or contractors use E-Verify. In 2007, the DHS began requiring all federal agencies to use the system. Arizona, in 2008, was the first state to mandate

⁵The penalties for I-9 noncompliance range from \$250 to \$5,500 per worker. In 2011, 2,740 audits resulted in total fines of over \$7 million.

⁶According to a Washington Times article, about 5% of all employees entered into the E-Verify system are determined to be unauthorized to work in the United States.

that all employers in the state use E-Verify as part of the Legal Arizona Worker's Act (LAWA). Seven other states (Alabama, Georgia, Mississippi, North Carolina, South Carolina, Tennessee, and Utah) have gone on to implement similar universal E-Verify mandates. Oftentimes, these universal mandates were slowly rolled into practice by making temporary exceptions for smaller firms. In some states, seasonal employees are also exempt. In total, 20 states have at some point mandated that either public employers or all employers use E-Verify. On the other end of the spectrum, California and Illinois have introduced legislation to limit employers' use of E-Verify.

Compliance with E-Verify mandates is determined through state audits. The penalties for employers found in noncompliance with mandated E-Verify use can range from civil fines to the revocation of business licenses. Even so, employer participation rates in states that universally mandate E-Verify was nowhere near 100% in 2017, though it was, on average, about 20 percentage points higher in these states than others.⁷

1.2.2 “Show Me Your Papers”

United States immigration law mandates that any non-citizen in the country for more than 30 days need to register with the federal government. Doing so will give them registration documents that they are required to carry at all times. Historically, local and state law enforcement have discouraged police officers from asking about immigration status. However, a handful of states have moved in the opposite direction by enforcing tough immigration laws that require police officers to attempt to determine the immigration status of suspected unauthorized immigrants. These laws are sometimes referred to as “Show Me Your Papers” (SMYP) laws.

⁷A 2017 study by the Cato Institute found that 27.8% of businesses use E-Verify in the top 10 E-Verify using states. This group includes every state with universal E-Verify mandates. Of the remaining, 40 states, only 6% of businesses use E-Verify, with no state having greater than 10% participation.

The first and most well-known state to pass a SMYP law was Arizona. Arizona SB 1070 was signed into law on April 23, 2010. At the time, it was considered to be the most comprehensive and strictest anti-illegal immigration policy ever passed in the United States. Among its many provisions, SB 1070 requires police officers to make efforts to determine a person's immigration status if there is reasonable suspicion that said person is unauthorized to be in the United States during any lawful stop or arrest. Any person found without proper identification is subject to arrest and detainment while his or her immigration status is determined.⁸ This will be referred to as a SMYP provision throughout this paper. SB 1070 also prohibits state and local law enforcement agencies from selectively choosing to not enforce federal immigration law. Other provisions included making it illegal for any person to knowingly conceal, harbor, or transport an undocumented immigrant or to induce an undocumented immigrant to immigrate to the state.

Arizona SB 1070 was scheduled to go into effect on July 29, 2010 but was almost immediately held up by numerous lawsuits questioning the constitutionality of its provisions. The potential for police officers to use racial profiling in determining reasonable suspicion that someone may be an undocumented immigrant was one of the major concerns opponents of SB 1070 used to challenge the law. The United States Justice Department filed its case on July 6, 2010, and blocked most of the provisions in SB 1070, including the SMYP provision, on July 28, 2010 in an Arizona District Court. Arizona governor Jan Brewer soon responded by filing an appeal. The case made its way to the United States Supreme Court, and on June 25, 2012, the Court reached the decision to strike down most of the provisions in SB 1070. Notably, however, all the SMYP actions described above were left intact and put into effect in Arizona shortly thereafter.

⁸Proper identification includes a valid Arizona driver's license, a valid tribal enrollment card, or other valid federal, state or local-government issued identification cards.

During the time Arizona SB 1070 was moving through the legal system, a handful of other states successfully passed similar legislation.⁹ The exact scope and severity of each of these laws varied, but SMYP provisions were common among them. In all of these cases, the laws were blocked immediately by civil rights groups and state justice departments after passage. The laws in two of these states, Indiana and Utah, never went into effect and still remain blocked. In two other states, Georgia and South Carolina, the state courts waited for the United States Supreme Court ruling on Arizona SB 1070. After the SMYP provision was upheld in Arizona, Georgia and South Carolina followed by upholding this portion of their laws while striking down many other portions. In Alabama, a district court temporarily blocked HB 56 on August 29, 2011, but upheld many of the key provisions, including SMYP, less than a month later, preempting the Supreme Court's decision on Arizona SB 1070. Of the four states that eventually enforced SMYP (Alabama, Arizona, Georgia, and South Carolina), only Georgia seems to have had issues with police officers actually enforcing SMYP. Most notably, the Atlanta Police Department decided to actively not enforce SMYP, citing reasons ranging from a desire to create a more welcoming environment to not prioritizing immigration status in efforts to reduce crime.¹⁰

1.3 Economic Framework

Consider the possible effects that E-Verify mandates and SMYP could have in a simple supply and demand model for undocumented labor. E-Verify mandates create an extra cost for employers hiring undocumented workers. This cost is due to potential penalties that the state will

⁹Even more states considered SMYP legislation, but it never made it past the voting stage.

¹⁰Other Georgia police departments known to not have complied with SMYP are the city of Sandy Springs, and Cobb, DeKalb, Fulton and Gwinnett counties. The Atlanta Journal-Constitution, May 25, 2014.

assess on an employer caught hiring undocumented workers during an audit. This increase in the expected cost of a new undocumented employee acts to reduce labor demand in this market. E-Verify mandates also can impose costs on undocumented workers looking for jobs. First, if an unauthorized immigrant is hired, she runs the risk of E-Verify determining that she is ineligible to work and alerting the DHS to her presence in the US. Second, there may be costs associated with obtaining fake social security numbers and photo identification documents that are required by E-Verify. These costs could encourage undocumented immigrants to leave the labor force (or even the state), reducing labor supply. These decreases in labor demand and labor supply should result in lower employment for undocumented workers. The effect on wages, however, will depend on the magnitude of the shifts in supply and demand. My empirical estimates on the wage impact of E-Verify can be used to test whether the shift in labor supply or demand is larger.

SMYP theoretically increases the incentives for undocumented workers to leave the state by increasing the chances of detainment and deportation. This should cause undocumented labor supply to decrease. SMYP might also impact labor demand for undocumented workers if employers are worried that their employees will be detained. Undocumented workers might be seen as riskier than other types of workers if they are less likely to show up and be productive at work on a daily basis. Thus, like E-Verify mandates, SMYP should theoretically result in a decrease in undocumented employment with an ambiguous effect on wages.

Given that both SMYP and E-Verify mandates are predicted to reduce the labor supply of undocumented workers, it is fair to wonder how this might impact other labor markets, particularly the market for low-skilled US citizens. If employers view undocumented workers and low-skilled citizens as labor substitutes, then labor demand for US citizens would likely increase, raising employment and wages. If, on the other hand, employers view undocumented workers

and low-skilled citizens more as labor complements, then labor demand for US citizens could fall, reducing employment and wages.

1.4 Data

My primary data source for demographic information and labor market outcomes is the American Community Survey (ACS) IPUMS 1% samples for 2005–2015. The ACS is a monthly cross-sectional survey administered by the U.S. Census Bureau. It covers all 50 states as well as the District of Columbia and Puerto Rico. The ACS contains detailed responses on race, ethnicity, education, income, employment, and a wide array of other demographic information. For analysis, I restrict the sample to the continental United States, excluding Georgia. As mentioned in Section 2.2, this is due to Georgia police departments only partially enforcing SMYP.¹¹

Following previous literature, I focus my analysis on the groups most likely to be impacted by immigration reform. The primary group of interest is unauthorized immigrant workers. Unfortunately, the ACS does not contain exact information about the legal-status of immigrants apart from US citizenship. However, utilizing widely-used methods based on Passel and Cohn (2014), I can proxy for unauthorized immigrants by examining the effects of SMYP and E-Verify mandates on the group of workers that is most likely to be undocumented. This group consists of low-skilled, working-age, non-citizen Hispanic immigrants. Due to their refugee status, Cuban immigrants are not included among the group of likely undocumented workers. Similarly, Puerto Ricans are also excluded from this group since they are US citizens. For ease, I will refer to this group of likely undocumented immigrants as NCI Hispanics. “Low-skilled” captures individuals with at most a

¹¹Including Georgia in my sample has little impact on my estimates of the effects of E-Verify mandates but does reduce the magnitude of my estimates of the impact of SMYP.

high school education.¹² I define “working-age” as 16-64.¹³ This definition of NCI Hispanics very likely overstates the true number of undocumented immigrants in the sample.¹⁴ Any bias introduced by the misclassification of some documented immigrants as unauthorized will bias my results for likely-undocumented workers towards zero, given that undocumented immigrants likely experience the largest labor market responses to SMYP and E-Verify mandates. It should also be noted that the ACS, as well as other nationally representative surveys such as the CPS, undercounts immigrants in the US, especially undocumented immigrants.¹⁵

In addition to looking at the impact of SMYP and E-Verify mandates on NCI Hispanics, I also consider the effects of these laws on three other groups: naturalized Hispanic citizens, US-born Hispanics, and US-born non-Hispanics.¹⁶ To better capture the most likely labor market competitors of NCI Hispanics, I restrict these groups to also be low-skilled adults between the ages of 16 and 64. Thus, these groups differ only across dimensions of race, citizenship, and immigration status. Naturalized Hispanics are Hispanic citizens who immigrated to the US, US-born Hispanics are non-immigrant Hispanic citizens, and US-born non-Hispanics are non-immigrant, non-Hispanic citizens.

The ACS does not ask individuals directly about their hourly wages. However, it does

¹²Restricting low-skilled workers to only those without a high school degree does not dramatically change my results.

¹³I do this to capture the age ranges used by previous studies on the effects of E-Verify. These range from 16-45 in Amuedo-Dorantes and Bansak (2014) to 20-64 in Orrenius and Zavodny (2015). My results are robust to a variety of age restrictions. See Table A.12 for more details.

¹⁴Passel and Cohn (2014) estimate that this group contains anywhere from 15-35% more potentially unauthorized immigrants than the actual number of unauthorized immigrants surveyed.

¹⁵Passel, Cohn, and Gonzalez-Barrera (2013) estimate that the ACS undercounts undocumented immigrants by 8-13% from 2005-2009 and by 5-7% from 2010 onward.

¹⁶Unlike previous literature, I include non-Hispanic black workers in my sample. While black adults make up a low percentage of the population in Arizona, the state which most literature has focused on, they are a very sizable percentage of the population in many states that implement SMYP and E-Verify laws.

contain real annual earnings, weeks worked per year, and hours worked per week. To calculate wage, I divide real annual income by weeks worked times hours worked per week. The weeks worked per year variable is measured categorically rather than continuously, so I use the midpoint of the range of weeks in each bin to estimate wages. All wages are adjusted to 2015 dollars.

I also collect data on various state-level outcomes and economic indicators. Data on state industry-specific GDP come from the Bureau of Economic Analysis (BEA) and are used in the analysis of the effects of SMYP and E-Verify mandates on sector productivity. Data on state government spending and state corporate income tax revenue come from the Census Annual Survey of State Government Finances (SGF). Data on state corporate income tax rates come from the Tax Policy Center. Data on new housing permits come from the Census Building Permit Survey. Data on state unemployment rates come from the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS) survey. Finally, data on detainments and removals by Immigration Customs Enforcement (ICE) come from the Transactional Records Access Clearinghouse (TRAC), and data on state-level crime rates come from the Uniform Crime Reports (UCR).

1.4.1 Descriptive Statistics

Table 1.1 shows select sample means for working-age, low-skilled men and women.¹⁷ These statistics highlight a few key differences between NCI Hispanics and their counterparts. NCI Hispanic men are the most attached group to the labor market, though their hourly wages are much lower than men with US citizenship. In contrast, NCI Hispanic women have the lowest employment and labor force participation. NCI Hispanics are also less educated and less likely to speak English. Additionally, NCI Hispanic men seem to be slightly more mobile than their

¹⁷These statistics are similar in magnitude to those found in previous studies using the CPS data.

Table 1.1: Summary Statistics

	NCI Hispanic (1)	naturalized Hispanic (2)	US-born Hispanic (3)	US-born non-Hispanic (4)
<i>men:</i>				
employed	0.791	0.790	0.582	0.651
real hourly wage	13.83	19.41	17.98	20.51
labor force participation	0.853	0.847	0.670	0.721
moved within last year*	0.042	0.018	0.028	0.027
age	36.05	44.03	32.63	39.66
high school degree	0.384	0.596	0.683	0.812
able to speak English**	0.473	0.754	0.967	0.998
disability	0.061	0.097	0.129	0.154
married	0.542	0.690	0.326	0.461
number of children	1.30	1.06	0.92	0.64
SMYP state	0.052	0.034	0.052	0.058
universal E-Verify state	0.100	0.055	0.072	0.144
public E-Verify state	0.228	0.284	0.209	0.381
Observations	425,848	179,045	636,289	5,906,927
<i>women:</i>				
employed	0.469	0.619	0.542	0.600
real hourly wage	11.16	15.65	15.55	16.72
labor force participation	0.540	.677	0.619	0.657
moved within last year*	0.029	0.015	0.024	0.024
age	37.39	44.56	33.56	40.86
high school degree	0.402	0.653	0.727	0.848
able to speak English**	0.382	0.716	0.965	0.998
disability	0.073	0.110	0.119	0.146
married	0.589	0.618	0.358	0.488
number of children	1.59	1.03	1.15	0.74
SMYP state	0.048	0.035	0.050	0.060
universal E-Verify state	0.087	0.052	0.070	0.150
public E-Verify state	0.209	0.279	0.206	0.387
Observations	367,097	194,239	624,174	5,847,968

Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The sample is restricted to low-skilled adults from ages of 16-64. NCI stands for “non-citizen, immigrant,” and is the group used to proxy for undocumented immigrants.

* = 1 if person reported migrating to their current location within the previous 12 months (including migration to the US)

** = 1 if person reported being able to speak English “very well” or “well”

counterparts, migrating more frequently, though this is in large part due to migration into the US. Finally, the fraction of NCI Hispanics living in states that go on to implement SMYP or E-Verify mandates is very comparable to those of their counterparts.

NCI Hispanics also tend to work in different sectors of the economy than their low-skilled counterparts.¹⁸ Among men, NCI Hispanics are most likely to work jobs in construction, manufacturing, entertainment and food services, professional and business services, and agriculture. Of these, only manufacturing and food services are comparably popular among low-skilled US citizen men. For women, NCI Hispanics are most likely to be employed in entertainment and food services, manufacturing, other services, professional and business services, and retail trade. Of these, only retail trade is more common among low-skilled US citizen women.

1.5 Methodology

1.5.1 Defining Treatment

For the bulk of my analysis, I consider three types of treatment: public E-Verify mandates, universal E-Verify mandates, and enforcing SMYP laws. Due to the ACS not having monthly observations, I treat all partially treated years as being fully treated years.¹⁹ I make this decision both for simplicity and to avoid having any treated observations listed among the control observations.²⁰ Any misclassification of control observations as treated in these partially treated years

¹⁸For a detailed breakdown of industries of employment by racial groups, please see Table A.3. For examples of the types of occupations included in each industry category, see Table A.4.

¹⁹For example, Alabama enacted SMYP on September 29, 2011. I code the year for Alabama's enacted SMYP treatment as 2011.

²⁰My results are robust to alternate ways of coding partially-treated years, including treating all partially treated years as fully treated years, rounding law enactment dates to the nearest January 1, and weighing the observations in partially treated years by the fraction of the year a law was in effect.

caused by the lack of monthly observations should bias the estimated impact of these immigration laws towards zero.

Figure 1.1 shows the different years that states have enacted the different policies. There is considerable variation across years that states have mandated public E-Verify and a fair amount with respect to universal E-Verify mandates. The lack of variation in years of SMYP enactment is potentially concerning if there are changes in other economic factors that influence labor market outcomes around this time. However, controlling for state and year fixed effects as well as factors that affect the business cycle helps mitigate some of these concerns.

Figure 1.1 also makes it clear that every state that mandates universal E-Verify also mandates public E-Verify. In order to clearly show the full impact of universal E-Verify mandates, I consider public E-Verify and universal E-Verify mandates to be mutually exclusive treatments. Thus, the coefficients on universal E-Verify mandates will show how these states differ from control states and not from states that mandate public E-Verify. I do allow for both SMYP and E-Verify mandates to be active in a state in the same year.

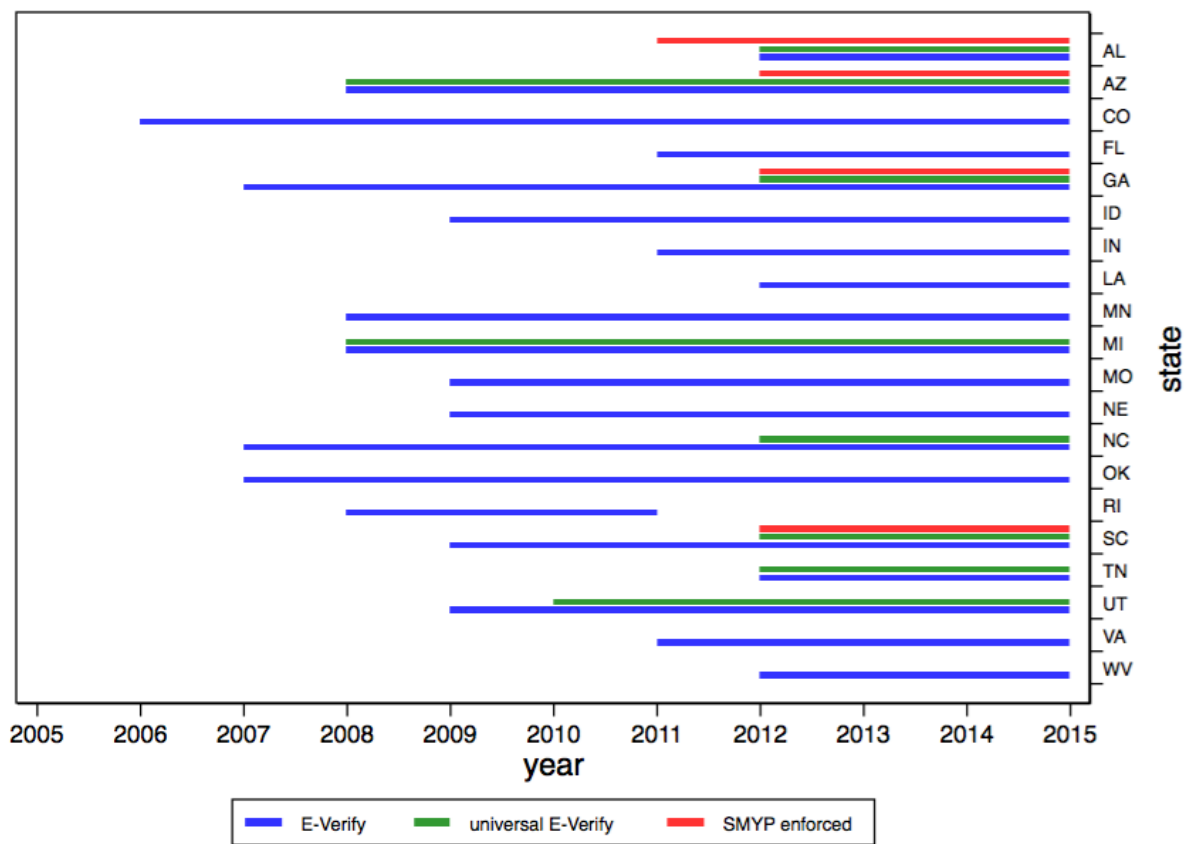
1.5.2 Estimating Equation

To examine the impact of different policies on the outcomes of different individuals, such as employment and wages, I use a difference-in-difference specification of the form:

$$Y_{inst} = \alpha + \beta SMYP_{st} + \gamma universal\ E-Verify_{st} + \delta public\ E-Verify_{st} + \eta X_{inst} + \theta Z_{st-1} + \mu_n + \mu_s + \mu_t + \epsilon_{inst} \quad (1.1)$$

Here, Y_{inst} is the outcome for person i , working in industry n and living in state s during year t . $SMYP_{st} = 1$ if state s has enforced SMYP at any time during year t and 0 otherwise.

Figure 1.1: Years of Immigration Reform Enactment



Source: Center for Immigration Studies

This figure shows the years in which E-Verify mandates and SMYP were implemented by different states.

*universal E-Verify*_{st} and *public E-Verify*_{st} are defined analogously for universal and public E-Verify mandates.²¹ X_{inst} is a vector of personal covariates.²² Z_{st-1} is a vector of lagged state-level covariates to control for business cycle effects including state unemployment rates, log state expenditure per capita, and new housing permits. Since state output could be impacted if immigration reform removes undocumented workers from the labor market, I do not include state GDP among my control variables.²³ The model also allows for industry, state, and year fixed effects. The coefficients of interest are β , γ , and δ , which show the effects of the three types of immigration reform on labor market outcomes.

The two main labor market outcomes I focus on are individual employment and log real hourly wages. I use this specification to examine several additional outcomes as well, including labor force participation and migration decisions. I run all individual-level regressions for men and women in each of the four main groups of interest (NCI Hispanics, naturalized Hispanics, US-born Hispanics, and US-born non-Hispanics) separately. Additionally, all regressions are weighted using the ACS person weights and standard errors are clustered at the state level.²⁴

Identification in this model relies on the assumption that a state's decision to adopt an immigration policy is independent of the current labor market conditions in that state. This is less of a concern if lawmakers enact such laws for more xenophobic or racial reasons, independent of how unauthorized immigrants or their likely low-skilled competitors are doing in the labor market.

²¹Note that *public E-Verify*_{st} = 0 if *universal E-Verify*_{st} = 1.

²²These include an age quartic polynomial, number of children, and dummies for race, sex, ability to speak English, marital status, and disability status.

²³Including state GDP is likely to understate the true impact of immigration reform on employment and wages due to the positive correlation between GDP and these outcomes. I include the other state-level covariates since they are not highly correlated with state GDP. Not including them in my model does not dramatically change my results, but adding state GDP as a control reduces the magnitude and significance of the estimated wage effects.

²⁴Running an unweighted regression results in very similar, but slightly larger in magnitude, results.

However, this could be problematic if lawmakers enact immigration reform in response to labor market changes due to the presence of unauthorized immigrants. To provide some evidence in support of the exogeneity of these policies, I run a series of simple regressions of the state-policy indicators on the observables.²⁵ None of the observables, including the population share of NCI Hispanics, predict that a state will implement either E-Verify mandates or SMYP in any economically significant way with the one exception of state government expenditures on public E-Verify mandates. For universal E-Verify and SMYP, the observables are not jointly significant, either. The lack of correlation between the observables and the policies makes the validity of the exogeneity assumption more plausible.²⁶ For my methodology to be valid, I must also assume parallel trends between the outcome variables in control states and treated states prior to the adoption of any laws. I provide evidence that this assumption does hold in section 6.4 when I examine the dynamic effects of these policies.

One concern that may bias my results is the possibility that the implementation of immigration reform might induce undocumented immigrants to misclassify themselves as US-citizens, non-Hispanic, or avoid surveys such as the ACS entirely. This does not appear to matter a great deal in the sample I use. The number of observations classified as naturalized Hispanics and US-born Hispanics do not noticeably increase in states that have SMYP or E-Verify mandates. The number of US-born non-Hispanics does go up slightly, but an undocumented immigrant would have to misclassify themselves across many dimensions to end up in this category. It seems unlikely that this would be a widespread issue.

Another concern is that my results could be biased if immigrants alter their migration pat-

²⁵See Figure A.5 for the detailed output of this analysis.

²⁶Using state fixed effects also helps control for state-specific unobservable factors.

terns and cluster in states without SMYP or E-Verify laws after these reforms are implemented. In such a scenario, workers in the control states might experience adverse labor market conditions from the increased competition. This would bias any negative results I find towards zero and any positive results I find upwards. If, on the other hand, these relocated unauthorized workers fill jobs that are not currently being occupied in the control states, then the overall economy of the control states could improve. This would likely overstate my findings. As discussed in Section 5.4, there is little evidence that immigrants migrate within the United States after either SMYP or E-Verify laws take effect. Additionally, Orrenius and Zavodny (2016) show that E-Verify does not increase the population of unauthorized immigrants in nearby control states. As such, this does not seem like a major concern when interpreting my results.

For certain outcomes, I can only look at state-aggregated values. For these regressions, I use the following model:

$$Y_{st} = \alpha + \beta SMYP_{st} + \gamma universal\ E-Verify_{st} + \delta public\ E-Verify_{st} + \theta Z_{st-1} + \mu_s + \mu_t + \epsilon_{st} \quad (1.2)$$

As with equation (1.1), analysis relies on the assumptions of exogeneity and parallel trends. State-level outcomes include total population and employment for each group of interest, ICE detainees, state corporate income tax revenue, and state GDP by industry. Standard errors are still clustered at the state level.

1.6 Results

I now present several key results from my analysis. Section 6.1 provides evidence that SMYP is actually enforced. Section 6.2 discusses the point estimates of these effects. Section 6.3 considers a model that allows for SMYP and E-Verify mandates to have complementary effects

on employment and wages when both are instituted in a state. Section 6.4 presents an event study of the effects of E-Verify mandates and SMYP on individual employment and wages. Section 6.5 examines the effects on state-aggregated employment and population. Section 6.6 presents evidence that SMYP and E-Verify mandates negatively impact states' overall productivity. Finally, Section 6.7 describes select robustness checks.

1.6.1 The Enforcement of SMYP

An implicit assumption in my analysis is that both E-Verify mandates and SMYP are actually enforced in states that implement these policies. While there is previous empirical evidence that universal E-Verify mandates lead to large, though incomplete, increases in E-Verify usage (see Bier (2017)), there has been little evidence on whether states that claim to enforce SMYP actually do enforce SMYP. This could be problematic since every state that implements SMYP also enforces universal E-Verify at some point. If SMYP is not actually enforced, then results I attribute to SMYP might, in reality, be due to more vigorous take-up of E-Verify in Alabama, Arizona, and South Carolina.²⁷

To provide some evidence that SMYP is impacting undocumented immigrants' decisions, I focus on an outcome that is much more likely to be impacted by SMYP than E-Verify mandates: Immigration Customs Enforcement (ICE) deportations. When state or local police hold a suspected unauthorized immigrant, ICE is notified and can issue a detainer requesting that law enforcement hold the suspect until an ICE agent arrives to take custody.²⁸ Law enforcement can choose whether

²⁷Bier (2017) reports that Alabama and Arizona have the two highest employer E-Verify participation rates.

²⁸ICE can be notified through a variety of cooperative programs with state and local law enforcement agencies. Some examples of these include 287(g), Secure Communities, the Criminal Alien Program (CAP), and the Mutual Agreement between Government and Employers (IMAGE) program.

or not to honor the detainer, but if ICE takes custody of the suspect, he or she will then have to go before an immigration court, possibly leading to deportation. If enforced, SMYP could increase the number of undocumented immigrants held by state and local law enforcement, which could in turn increase the number of detainer requests ICE issues and ultimately, the number of deportations ICE makes.

To examine ICE-related outcomes, I use data from ICE collected via a Freedom of Information Act request made by the Transactional Records Access Clearinghouse (TRAC). The TRAC data allow me to observe the number of detainer requests ICE issues in each state and each year from 2005-2014. I can also observe the number of detainer requests made for individuals with no criminal charges, the number of individuals ICE takes custody of, and the number of individuals ICE removes from the US at the state-year level. In analyzing the effects of E-Verify mandates and SMYP on these outcomes, I also control for state violent crime and property crime rates from the Uniform Crime Reports (UCR) to adjust for the potential that increasing crime rates may increase the number of interactions between undocumented immigrants and law enforcement officials.²⁹

The results of this analysis, as seen in Table 1.2, show very large, though imprecise, point estimates for the increase in ICE activity in SMYP states. ICE detainer requests increase by 67.8% in states that implement SMYP, even after controlling for state crime rates. Detainers issued for individuals without criminal charges increase by a similar, though marginally insignificant, percentage. Furthermore, ICE increases the number of suspected aliens they take custody of by over 80%. Interestingly, SMYP does not increase the actual number of removals ICE makes. One reason for this is that removals not only include individuals deported via state and local law en-

²⁹The charge for not having proper documentation is a misdemeanor and would not affect these crime rates.

Table 1.2: ICE Detainers and Removals Results

	detainers issued (all) (1)	log ICE activity detainers issued (no criminal charges) (2)	custody taken (3)	removals made (4)
SMYP	0.678* (0.378)	0.670 (0.429)	0.810*** (0.294)	0.012 (0.237)
universal E-Verify	-0.054 (0.357)	-0.008 (0.424)	0.179 (0.154)	0.120 (0.312)
public E-Verify	0.311 (0.230)	0.316 (0.269)	0.221 (0.246)	0.132 (0.249)
Observations	480	480	480	480
Year FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓

Notes: 2005 - 2014 Immigration Customs and Enforcement (ICE) data collected from TRAC and Uniform Crime Reports data. Controls include state violent crime rates and property crime rates by year. Standard errors clustered at the state level in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

forcement, but also deportations made by the US Border Patrol. Border Patrol removals are not likely to increase in response to SMYP and may even decrease given that SMYP reduces the inflow of NCI Hispanics.³⁰ Neither form of E-Verify mandates has any significant impact on ICE activity within a state. This provides a plausible channel that SMYP could operate through to affect the decisions and outcomes of undocumented immigrants independently of universal E-Verify mandates. A likely mechanism behind this is that police officers in SMYP states are, in fact, checking documentation more frequently and holding more undocumented immigrants.

³⁰Hoekstra and Orozco-Aleman (2017) show that the flow of undocumented workers into Arizona decreases by as much as 70% following SMYP.

1.6.2 Individual Employment and Wage Results

Recall from Section 3 that the basic labor supply and demand model predicts that both SMYP and E-Verify mandates will unambiguously reduce employment for NCI Hispanics. The predicted effects on wages, however, are less clear since both policies may induce decreases in labor supply and labor demand. To test these predictions, I estimate the impact of SMYP and E-Verify mandates on individual-level employment and wages using the model specified in equation (1.1). The results of this analysis are displayed in Table 1.3.

Since both SMYP and E-Verify mandates directly target undocumented immigrants, it is natural to expect that NCI Hispanic workers would experience the largest employment and wage responses. Indeed this is the case. Employment rates for NCI Hispanic men decrease by 1.8 percentage points in response to SMYP and by 3.4 percentage points in response to universal E-Verify mandates. Likewise, real hourly wages for NCI Hispanic men decrease by 1.7% and 2.7% due to SMYP and universal E-Verify mandates respectively. Even public E-Verify mandates, which affect a smaller percentage of the population, have large negative effects on NCI Hispanic men's employment and wages, though these effects are smaller in magnitude than those for universal E-Verify mandates.³¹ These results are consistent with the predictions from the supply and demand framework, and the wage declines are suggestive that both SMYP and E-Verify mandates considerably decrease labor demand for NCI Hispanic men.

The results for NCI Hispanic women paint a different picture. This is not entirely unexpected, since among NCI Hispanics, women are much less attached to the labor force than men and

³¹Even though NCI Hispanics do not work in public sector jobs, public E-Verify mandates could still impact their employment and wage prospects since firms that contract with public sector jobs are also impacted by this reform. Additionally, public E-Verify mandates may raise awareness of E-Verify among private firms, resulting in some voluntary take up.

Table 1.3: Employment and Wage Results

	men		women	
	employment (1)	log wage (2)	employment (3)	log wage (4)
<i>NCI Hispanics:</i>				
SMYP	-0.018** (0.008)	-0.017* (0.009)	0.001 (0.004)	0.015 (0.016)
universal E-Verify	-0.034*** (0.011)	-0.027** (0.012)	-0.007 (0.005)	0.023* (0.011)
public E-Verify	-0.020*** (0.005)	-0.020** (0.009)	-0.009** (0.004)	-0.017 (0.014)
Observations	425,261	340,942	366,626	180,095
<i>naturalized Hispanics:</i>				
SMYP	0.016 (0.015)	0.034* (0.020)	0.012 (0.016)	-0.058*** (0.014)
universal E-Verify	-0.013* (0.007)	-0.013 (0.017)	-0.017 (0.013)	0.055*** (0.019)
public E-Verify	-0.004 (0.006)	-0.005 (0.010)	0.005 (0.003)	-0.008 (0.011)
Observations	179,303	141,641	194,053	123,472
<i>US-born Hispanics:</i>				
SMYP	-0.000 (0.003)	0.007 (0.013)	-0.008** (0.003)	0.041*** (0.010)
universal E-Verify	-0.020*** (0.004)	-0.008 (0.010)	0.000 (0.008)	-0.013 (0.011)
public E-Verify	-0.007 (0.004)	-0.005 (0.014)	-0.003 (0.003)	-0.001 (0.010)
Observations	635,958	419,018	623,855	385,069
<i>US-born non-Hispanics:</i>				
SMYP	-0.005*** (0.001)	0.009 (0.006)	-0.001 (0.002)	0.012** (0.005)
universal E-Verify	-0.005** (0.002)	0.000 (0.008)	0.000 (0.002)	-0.004 (0.006)
public E-Verify	-0.002 (0.002)	0.000 (0.006)	-0.001 (0.001)	0.004 (0.004)
Observations	5,898,023	4,142,344	5,837,938	3,861,050
Year FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓

Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The coefficients on employment measure the percentage point change in the number employed divided by the total population (including those not in the labor force). The model controls for individual level characteristics and state level business cycle variables. Standard errors clustered at the state level in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

typically work in different sectors of the economy. As such, their employment is hardly affected by SMYP or E-Verify mandates. Despite this, NCI Hispanic women see a 2.3% wage increase in states that enact universal E-Verify mandates. This increase might well be explained by the fact that NCI Hispanic women are more likely to work in private households or businesses employing very few workers, such as small restaurants, that are exempt from these E-Verify mandates. Hence, the labor demand for NCI Hispanic women could remain unchanged or even increase.

Given the sizable decreases in employment among NCI Hispanic men in states that enact tough immigration reform, it stands to reason that a large number of jobs previously occupied by these men would now be vacant for their low-skilled counterparts to fill. It is surprising then that neither SMYP nor E-Verify mandates increase the employment percentages of naturalized Hispanic, US-born Hispanic, or non-Hispanic men. In fact, universal E-Verify mandates decrease employment by 1.3 percentage points for naturalized Hispanic men and 2.0 percentage points for US-born Hispanic men. This could be indicative that low-skilled US citizens are not particularly close labor substitutes for undocumented workers, especially in industries like construction that are more likely to be impacted by universal E-Verify mandates. Low-skilled US citizen men also do not experience wage changes due to immigration reform, with the one exception of naturalized Hispanic men in states that implement SMYP. They receive a 3.4% wage bump. This might suggest that these men might be more substitutable for NCI Hispanics in industries, such as agriculture, that might not be heavily impacted by E-Verify mandates, but could be affected by SMYP.³²

Low-skilled, citizen women do not see any sizable employment changes, much like their NCI Hispanic counterparts. However, they do experience some rather large wage fluctuations,

³²Agricultural employers tend to hire seasonal workers or few workers in total. Both these practices are exempt from universal E-Verify mandates in most states.

particularly in response to SMYP. This could be related to the types of jobs and sectors these women work, which tend to be very different from the industries that NCI Hispanic women work.

1.6.3 The Combined Effects of SMYP and Universal E-Verify Mandates

Three states (Alabama, Arizona, and South Carolina) have enforced both SMYP and universal E-Verify mandates.³³ Since these laws operate through different channels (law enforcement for SMYP and employer-run background checks for E-Verify), they could interact and complement each other in a variety of ways. First, SMYP could have no labor market effects beyond those of universal E-Verify mandates. That is, the NCI Hispanics who would be affected by SMYP are the same ones already impacted by universal E-Verify mandates. In this case, the employment and wage effects in these states should be roughly the same as in states that only have universal E-Verify mandates. Second, SMYP could increase the labor market effects beyond those of E-Verify mandates. In this case, SMYP would cause stronger reactions from NCI Hispanics or their potential employers than universal E-Verify mandates would alone. If these laws complement each other particularly well, then these combined effects on employment and wages could be even larger than the sum of their individual effects.

To test for such complementarity, I use a slight variation of equation (1.1) where I consider four mutually exclusive treatment types: states with only SMYP active in a given year, states with only universal E-Verify mandates active in a given year, states with only public E-Verify mandates active in a given year, and states with both SMYP and universal E-Verify mandates active in a given year. The key results of this specification are shown in Table 1.4.³⁴

³³Georgia also has, but as mentioned before, observations from Georgia are not included in my sample.

³⁴The “public E-Verify only” treatment coefficients are not included in Table 1.4 due to their extreme similarity to

Table 1.4: Employment and Wage Results (Combined Effects)

	men		women	
	employment (1)	log wage (2)	employment (3)	log wage (4)
<i>NCI Hispanics:</i>				
SMYP & universal E-Verify	-0.053*** (0.014)	-0.043*** (0.010)	-0.007 (0.006)	0.034*** (0.012)
universal E-Verify only	-0.027*** (0.010)	-0.024* (0.012)	-0.003 (0.004)	0.030*** (0.011)
Prob > F	0.002***	0.046**	0.413	0.730
Observations	425,261	340,942	366,626	180,095
<i>naturalized Hispanics:</i>				
SMYP & universal E-Verify	0.004 (0.011)	0.019 (0.018)	-0.006 (0.015)	-0.005 (0.029)
universal E-Verify only	-0.015** (0.006)	-0.010 (0.016)	-0.021 (0.013)	0.058*** (0.019)
Prob > F	0.108	0.158	0.308	0.001***
Observations	179,303	141,641	194,053	123,472
<i>US-born Hispanics:</i>				
SMYP & universal E-Verify	-0.022*** (0.006)	-0.004 (0.020)	-0.008 (0.006)	0.028 (0.018)
universal E-Verify only	-0.016*** (0.004)	-0.005 (0.008)	0.002 (0.008)	-0.012 (0.011)
Prob > F	0.324	0.961	0.012**	0.002***
Observations	635,958	419,018	623,855	385,069
<i>US-born non-Hispanics:</i>				
SMYP & universal E-Verify	-0.010*** (0.003)	0.007 (0.008)	-0.001 (0.002)	0.008 (0.008)
universal E-Verify only	-0.004** (0.002)	0.001 (0.007)	0.001 (0.002)	-0.003 (0.006)
Prob > F	0.002***	0.241	0.396	0.015**
Observations	5,898,023	4,142,344	5,837,938	3,861,050
Year FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓

Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The coefficients on employment measure the percentage point change in the number employed divided by the total population (including those not in the labor force). Prob > F shows the p-value for the F-test that the coefficient on “universal E-Verify only” is equal to the coefficient on “SMYP & universal E-Verify.” The model controls for individual level characteristics and state level business cycle variables. The model also includes dummies for the “SMYP only” and “public E-Verify only” treatments. Standard errors clustered at the state level in parentheses: *** p < 0.01, ** p < 0.05, * p < 0.1

This analysis reveals two key findings. First, the combined effects of SMYP and universal E-Verify mandates are nearly identical to the sum of the effects of SMYP and universal E-Verify mandates shown in Table 1.3. Thus, there is no evidence of any additional complementarities in the combined effects of these laws. Second, in several cases, the hypothesis of the equality of the combined effect of SMYP and universal E-Verify mandates and the effect of universal E-Verify mandates alone is soundly rejected. This most notably occurs for the employment and wages of NCI Hispanic men. These results show that SMYP does have an impact on the labor market beyond that of universal E-Verify mandates. This finding could suggest that previous studies may suffer from omitted variable bias by not controlling for states that implement SMYP. However, analysis that omits the SMYP treatment does not significantly alter the point estimates for the effects of either universal E-Verify mandates or public E-Verify mandates.³⁵ Thus, omitted variable bias does not seem to be a major concern when analyzing these laws.

1.6.4 The Dynamic Effects of SMYP and E-Verify Mandates

So far, my estimates point to both SMYP and E-Verify mandates significantly reducing NCI Hispanic men's employment and wages. However, more can be learned about the nature of these effects if we allow them to vary over time. To examine these dynamic effects of immigration reform, I run a simple event study of the effects of SMYP and E-Verify mandates on individual

the estimated effects in Table 1.3. The "SMYP only" treatment coefficients are also not reported. This is because only one state in one year, Alabama in 2011, fits this treatment definition.

³⁵Table A.6 shows these results fully. With the exception of naturalized Hispanic wages, the potential bias in the estimated effects of universal E-Verify mandates is less than 0.5 percentage points on employment and 0.5% on wages.

employment and log wages. This analysis takes the following form:

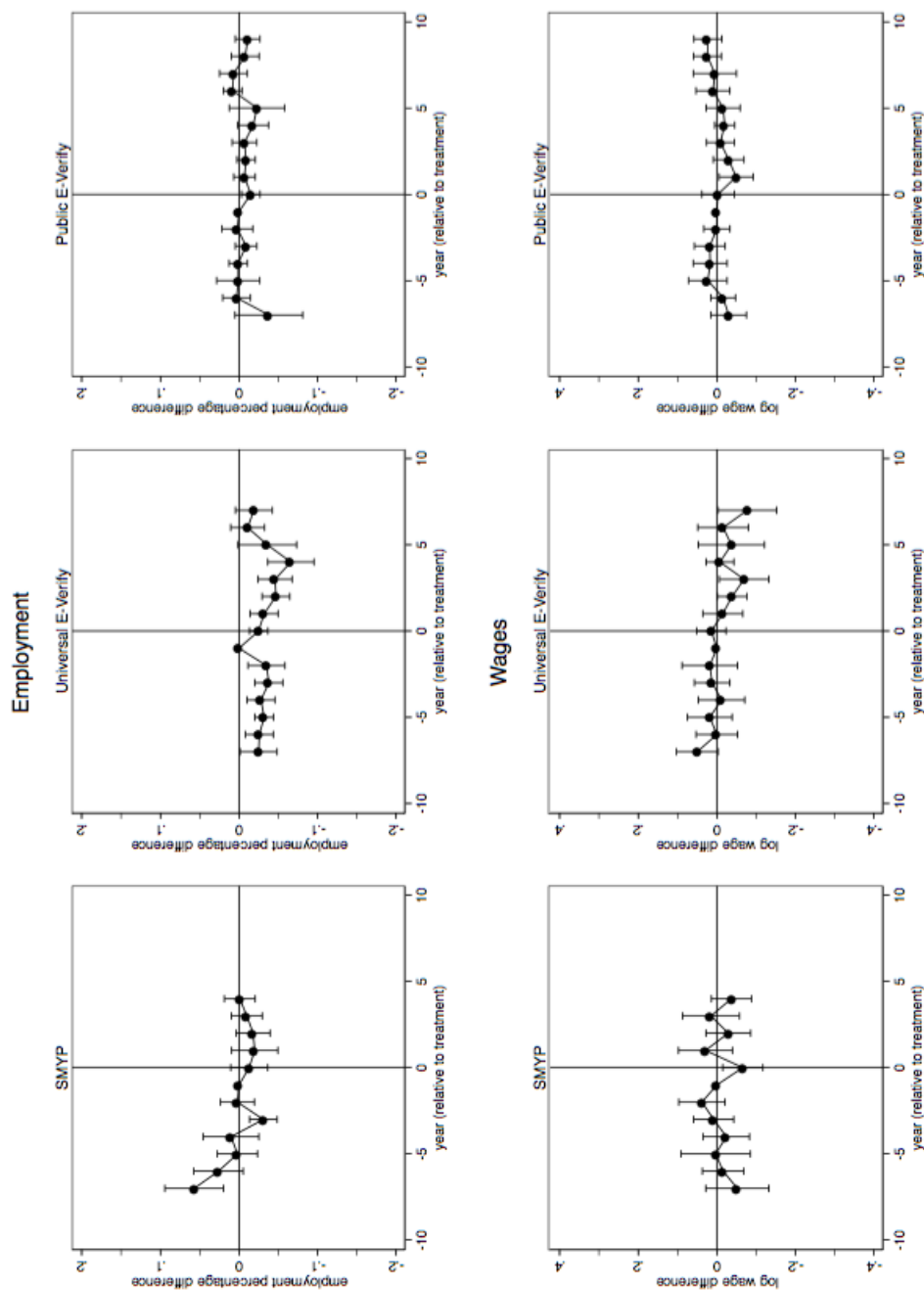
$$Y_{ist} = \alpha + \sum_{\tau \neq -1} \beta_{\tau} SMYP_{s\tau} + \gamma_{\tau} universal\ E-Verify_{s\tau} + \delta_{\tau} public\ E-Verify_{s\tau} + \eta X_{inst} + \theta Z_{st-1} + \mu_n + \mu_s + \mu_t + \epsilon_{ist} \quad (1.3)$$

Y_{ist} is the labor market outcome for person i in state s in calendar year t , and μ_s and μ_t are state and year fixed effects respectively. $SMYP_{s\tau}$ is a vector of dummy variables that take a value of 1 if state s has ever enforced SMYP and the observation is τ years from the year SMYP is enforced. *universal E-Verify* $_{s\tau}$, and *public E-Verify* $_{s\tau}$ are defined analogously for states that have implemented universal E-Verify mandates and public E-Verify mandates respectively. As before, X_{inst} is a vector of personal covariates, and Z_{st-1} is a vector of lagged state-level covariates. The coefficients of interest are the β_{τ} 's, γ_{τ} 's, and δ_{τ} 's, which are plotted with 95% confidence intervals to visually display the dynamic effects of each of the three immigration reforms.³⁶

Figure 1.2 plots the employment percentage and log wage coefficients for NCI Hispanic men for SMYP, universal E-Verify mandates, and public E-Verify mandates. Several interesting patterns emerge. First, there are no strong pre-trends for universal or public E-Verify mandates. This lends validity to the results obtained using the difference-in-difference approach in my main specification. Similarly, there is no strong pre-trend for SMYP on NCI Hispanic men's wages. However, there seems to be a slight downward trend in their employment prior to the implementation SMYP. This might be picking up some dynamics with regards to passing SMYP without enforcement. It can also be noted that the pre-trend in employment settles around zero within five years of the enforcement of SMYP.

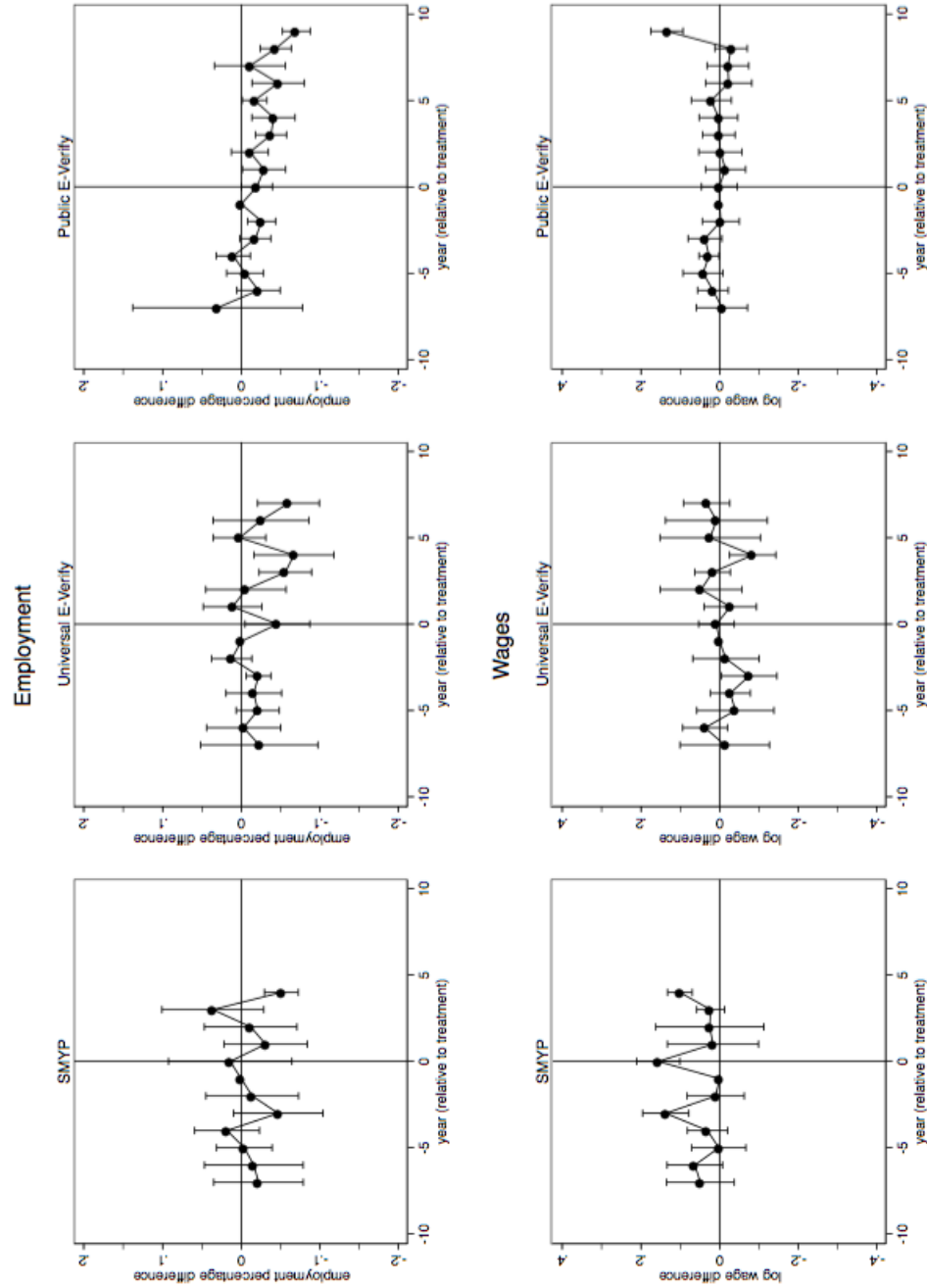
³⁶Estimating dynamic effects could also be done via a series of three regressions that each allow one policy to have dynamic effects while fixing the other two to have static effects (i.e. = 1 in all years that the policy is in effect and 0 otherwise). This alternate specification leads to very similar estimates.

Figure 1.2: NCI Hispanic Dynamic Effects (Men)



Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The sample is restricted to low-skilled, noncitizen immigrant Hispanic men from ages of 16-64. The graphs plot the differences in the estimated effects of SMYP, universal E-Verify mandates, and public E-Verify mandates on employment percentage and log real hourly wages between individuals in the affected states and control states relative to a law implementation year of 0. These estimates net out state and year fixed effects. See section 5.3 for more details.

Figure 1.3: NCI Hispanic Dynamic Effects (Women)



Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The sample is restricted to low-skilled, noncitizen immigrant Hispanic women from ages of 16-64. The graphs plot the differences in the estimated effects of SMYP, universal E-Verify mandates, and public E-Verify mandates on employment percentage and log real hourly wages between individuals in the affected states and control states relative to a law implementation year of 0. These estimates net out state and year fixed effects. See section 5.3 for more details.

In terms of employment percentage, universal E-Verify mandates seem to have larger and longer-lasting negative effects than either public E-Verify mandates or SMYP. While the employment effects remain negative even five years after universal E-Verify mandates, they have more or less returned to pre-implementation levels for public E-Verify mandates and SMYP by then. The post-reform dynamic wage effects are noisier, but still support the idea that the effects of universal E-Verify mandates are longer lasting. Both SMYP and public E-Verify cause one-to-two year declines in wages for NCI Hispanic men that disappear over time (and may eventually become positive in the case of public E-Verify mandates). Universal E-Verify mandates, on the other hand, may still have negative wage effects as far out as seven years after implementation, though this is not conclusive.

The dynamic effects for NCI Hispanic women in Figure 1.3 are much noisier than those for men. Still some patterns emerge. The post-reform effects for employment are fairly similar across all three types of reforms. This could be due to NCI Hispanic women having a lower labor force attachment than men. There also seems to be some positive wage effects on women for SMYP and (suggestively) for universal E-Verify mandates. This is consistent with the point estimates, and as discussed earlier, can most likely be attributed to women working in industries, such as private households, that are less impacted by immigration reform. Apart from the very last observable post-treatment years, these wage effects do not appear to be persistent.³⁷ As with NCI Hispanic men, prior to the implementation of immigration reform, there is not much noticeable difference in the trends in treated and control states.

³⁷These large positive spikes in wages (and negative spikes in employment) are due to only one state being captured in these years for these laws. Only Alabama has SMYP active for four years, and only Colorado has public E-Verify active for nine years. As such, it may be more accurate to ignore these two points throughout in this dynamic analysis.

The event studies for US citizens are shown in Figures A1–A6 and reveal a few notable patterns. First US-born non-Hispanics seem almost completely unaffected by SMYP or E-Verify mandates. The same can pretty much be said for US-born Hispanics, though US-born Hispanic men might experience reduced employment following universal E-Verify mandates. The trends for naturalized Hispanics are much noisier, but there is some evidence that naturalized men might see small and short-lived (ignoring the final post-treatment year of observations) gains in wages following SMYP or universal E-Verify mandates. As before, universal E-Verify mandates seem to have much longer lasting impacts than public E-Verify mandates or SMYP for US citizens.

1.6.5 Aggregate Employment and Population Results

Recall that for NCI Hispanic men, universal E-Verify mandates reduce the employment rate of the low-skilled, working-age population by 3.4 percentage points. But this finding does not shed any light on whether this policy (or SMYP or public E-Verify mandates) affects the size of the NCI Hispanic population. To provide evidence on this particular question, I run a model using estimating equation (1.2) with aggregate employment and population at the state level as the outcome variables.³⁸

The results of this estimation are presented in Table 1.5. All three types of immigration reforms result in large decreases in the employment of NCI Hispanic men and women. NCI Hispanic men are hit harder by E-Verify mandates than NCI Hispanic women with a nearly 20% decrease in total employment following universal E-Verify mandates. However, both men and women experience a roughly 14% decrease in employment after SMYP takes effect. This can be taken as

³⁸To calculate aggregate employment, I sum all the individuals that are employed by state, year, Hispanic-type, and sex. I calculate aggregate population by summing all the individuals by state, year, and Hispanic-type. These regressions are weighted by a sum of the person weights.

Table 1.5: Aggregate Employment and Population Results

	employment		population		
	men	women	men	women	total
	(1)	(2)	(3)	(4)	(5)
<i>NCI Hispanics:</i>					
SMYP	-0.145*** (0.034)	-0.137*** (0.021)	-0.073* (0.038)	-0.134*** (0.020)	-0.103*** (0.026)
universal E-Verify	-0.196*** (0.056)	-0.059 (0.043)	-0.143*** (0.032)	-0.044 (0.044)	-0.101*** (0.035)
public E-Verify	-0.100*** (0.019)	-0.065*** (0.023)	-0.076*** (0.019)	-0.041 (0.025)	-0.061*** (0.020)
Observations	514	513	514	513	514
<i>naturalized Hispanics:</i>					
SMYP	-0.027 (0.024)	0.001 (0.036)	-0.034 (0.029)	-0.034 (0.029)	-0.032 (0.028)
universal E-Verify	0.034 (0.045)	0.058 (0.076)	0.056 (0.046)	0.082 (0.061)	0.068 (0.053)
public E-Verify	0.008 (0.017)	0.044** (0.021)	0.024 (0.018)	0.030 (0.019)	0.028 (0.017)
Observations	510	513	510	513	513
<i>US-born Hispanics:</i>					
SMYP	-0.009 (0.029)	-0.022 (0.022)	0.001 (0.025)	-0.023 (0.021)	-0.011 (0.022)
universal E-Verify	0.013 (0.059)	0.014 (0.046)	0.061 (0.053)	0.048 (0.040)	0.055 (0.046)
public E-Verify	-0.035 (0.030)	-0.022 (0.029)	-0.015 (0.032)	-0.018 (0.032)	-0.016 (0.032)
Observations	517	517	517	517	517
<i>US-born non-Hispanics:</i>					
SMYP	0.002 (0.011)	-0.000 (0.007)	0.019 (0.014)	0.006 (0.009)	0.013 (0.011)
universal E-Verify	0.016 (0.010)	0.026*** (0.008)	0.023** (0.009)	0.027*** (0.009)	0.024** (0.009)
public E-Verify	0.012** (0.005)	0.014*** (0.005)	0.013** (0.006)	0.013** (0.005)	0.013** (0.005)
Observations	517	517	517	517	517
Year FE	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓

Notes: 2005 - 2015 American Community Survey (ACS) data aggregated to the state-year level. The model controls for state level business cycle variables. Standard errors clustered at the state level in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

evidence that SMYP affects NCI Hispanics across all industries while universal E-Verify mandates effectively only target certain industries. Also notice that the total population of NCI Hispanics decreases by about 10% in states that implement SMYP or universal E-Verify mandates. The decrease is a smaller, but still significant, 6% in public E-Verify states. E-Verify mandates have noticeably smaller effects on the size of the population of NCI Hispanic women than SMYP, again suggesting that industries that typically employ undocumented women are more likely to be exempt from E-Verify mandates. These population decreases are entirely consistent with a reduction in the labor supply of NCI Hispanics as there are now fewer available to work in the treated states. This notion is further supported by examining the labor force participation of NCI Hispanics. Table A.7 describes these results in detail, but the key takeaway is that the decreases in the employment percentages of NCI Hispanic men are mostly explained by a reduction in their labor force participation. Not only is the total population of NCI Hispanics decreasing, but they are participating in the labor force at lower rates. This results in an unambiguous decrease in labor supply.

If the NCI Hispanic populations in SMYP and E-Verify states decrease, where are these displaced workers going? A quick look at the migration habits of low-skilled workers provides some answers. Table A.8 shows that NCI Hispanics (and low-skilled workers in general) are less likely to have moved to a state that has implemented E-Verify mandates but are not more likely move out of an affected state to another state.³⁹ Additionally, it does not seem like NCI Hispanics move from SMYP states to non-SMYP states.⁴⁰ This suggests that the decrease in NCI Hispanic population is most likely to be explained by NCI Hispanics leaving the US, an outcome I cannot

³⁹These measures are calculated from an ACS question asking where an individual lived 12 months ago.

⁴⁰This result is consistent with the prior literature, including Orrenius and Zavodny (2016), and helps mitigate concerns that undocumented immigrants cluster in control states after the implementation of SMYP and E-Verify mandates. If this clustering were going on, we should expect to see an increase in the migration from treated states. This is not the case, though individuals who are choosing to migrate are more likely to move to control states.

observe in the ACS.⁴¹

The decreases in the total number NCI Hispanics employed shown in Table 1.5 offer evidence that SMYP and E-Verify mandates result in job vacancies that could potentially be filled by other low-skilled workers, but, as in the analysis of Section 6.3, it does not appear that other low-skilled workers fill these jobs. Total employment for naturalized Hispanic, US-born Hispanic, and non-Hispanic men pretty much remains unchanged.⁴² There appear to still be many jobs that either remain unfilled or simply do not exist anymore after NCI Hispanics leave a state's labor force in response to immigration reform.

1.6.6 State-Level Economic Results

Both SMYP and E-Verify mandates drastically reduce the total employment of NCI Hispanics. By and large, the total employment of other low-skilled workers remains unchanged. These results suggest that both SMYP and E-Verify mandates lead to unfilled job vacancies. This could have a variety of negative effects for firms doing business in the affected states. Perhaps the most obvious possibility is that a decrease in workers leads to a decrease in total productivity, particularly in industries that traditionally rely more heavily on undocumented labor. To test this notion, I run a regression using estimating equation (1.2) of per capita state GDP on a one-year lag of state unemployment rates and state and year fixed effects.⁴³

⁴¹Caballero, Cadena, and Kovak (2017) do show that E-Verify mandates in Arizona increased return migration to Mexico.

⁴²Additionally, the total populations of naturalized and US-born Hispanics do not increase. This helps to mitigate concerns that undocumented immigrants might be systematically changing their responses to ACS survey questions about their citizenship or immigrant status after SMYP or E-Verify mandates are implemented.

⁴³State unemployment rates are correlated with state GDP and may also be impacted by immigration reform. The decision to include or not include them has very minimal impacts on the point estimates.

Table 1.6: State GDP Results

	log per capita GDP					
	total	agriculture	construction	manufacturing	prof. services	food services
	(1)	(2)	(3)	(4)	(5)	(6)
SMYP	-0.024 (0.020)	-0.119** (0.047)	-0.142 (0.090)	0.028 (0.030)	-0.042** (0.019)	-0.015 (0.027)
universal E-Verify	-0.031** (0.013)	0.031 (0.063)	-0.101** (0.048)	0.046 (0.034)	-0.001 (0.017)	-0.029*** (0.010)
public E-Verify	-0.019* (0.011)	0.028 (0.048)	-0.036 (0.040)	0.036 (0.031)	-0.031* (0.016)	-0.019** (0.009)
Observations	528	520	528	528	528	528
Year FE	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓

Notes: 2005 - 2015 real state GDP from the Bureau of Economic Analysis (BEA). Controls include a 1-year lagged state unemployment rate. Standard errors clustered at the state level in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The results of this analysis are stunning. Table 1.6 shows that E-Verify mandates and SMYP laws not only have large impacts on industries that typically employ undocumented workers, but also negatively impact overall state productivity.⁴⁴ In particular, universal E-Verify mandates are associated with an over 3% reduction in per capita state GDP, and even public E-Verify mandates lead to a nearly 2% decrease. Though the effect of SMYP on state GDP is not statistically different from zero, the magnitude is well in-range of the E-Verify effects.

It is also well worth pointing out that these effects can be particularly strong in industries that employ large numbers of unauthorized workers. For instance, universal E-Verify mandates result in a roughly 10% reduction in state construction GDP per capita, and SMYP results in a nearly

⁴⁴These findings are consistent with anecdotal evidence about the effects of these laws on state productivity. For instance, in Alabama, the agriculture industry suffered mightily as farmers were unable to find workers to assist with picking crops. State plans to use work release inmates to assist farmers were unsuccessful.

Source: <https://www.npr.org/2011/10/24/141638999/labor-worries-rise-as-planting-season-nears-in-ala>

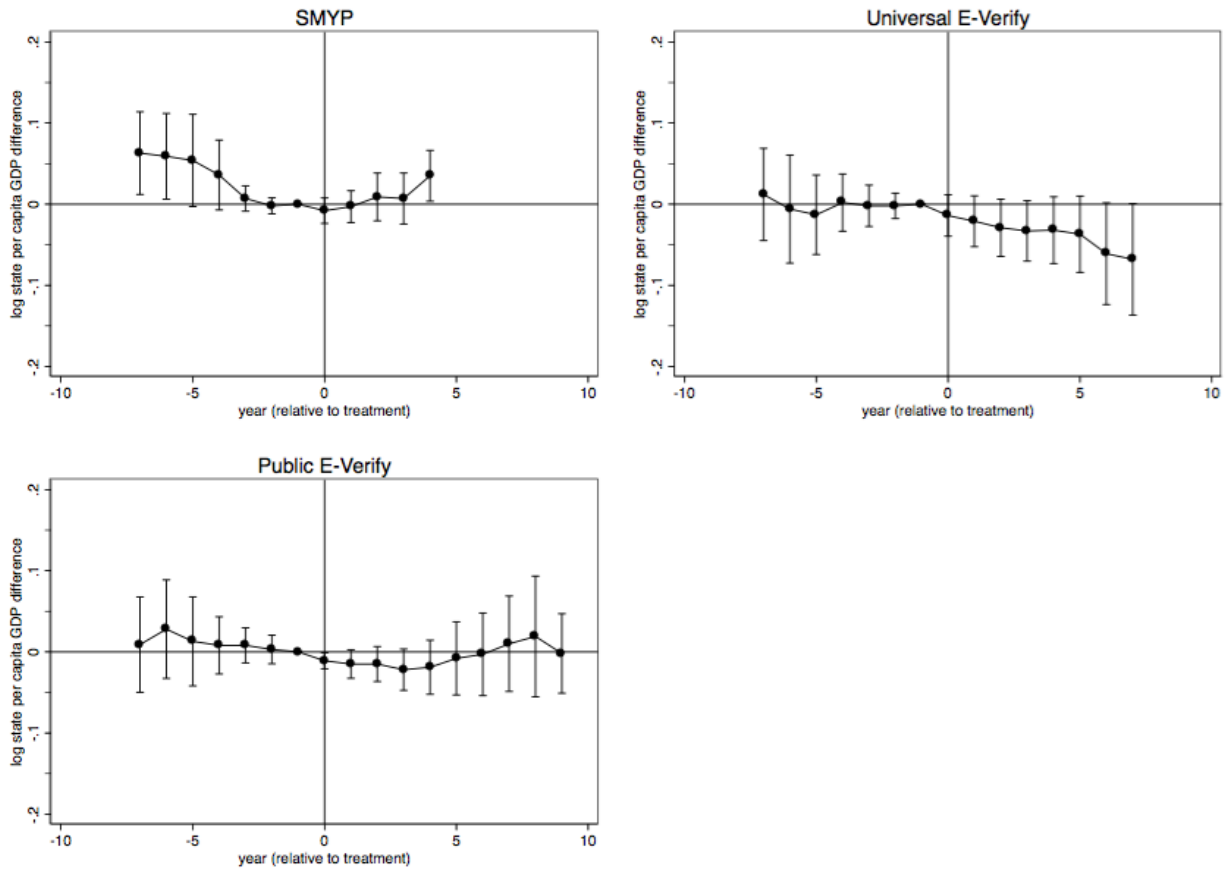
12% decrease in state agriculture GDP per capita. Interestingly, neither type of E-Verify mandate has any noticeable effect on agriculture productivity. A likely explanation is that agricultural employers are typically exempt from universal E-Verify mandates, but undocumented agricultural workers are still impacted by SMYP regulations. Thus it seems that SMYP may have a broader impact on the population and industries within a state, but universal E-Verify could have a larger impact in the industries that are targeted.

To address possible concerns about the validity of my interpretation that immigration reform causes a decrease in state GDP, I run an event study analogous to the one in section 6.4, though in this case, the data is at the state level and not the individual level. Figure 1.4 plots these dynamic effects. Apart from a downward trend prior to the implementation of SMYP, the pretrends on state GDP look to be parallel and centered around zero.⁴⁵ This is consistent with the notion that at the very least, E-Verify mandates are responsible for a decrease in GDP. Also notice that the post-law effects for universal and public E-Verify mandates are especially interesting. As with individual employment and wages, universal E-Verify mandates result in a long-lasting decline in GDP per capita following implementation. Public E-Verify mandates result in a dip in GDP that lasts for a few years before returning to pre-implementation levels. Employers seem to be better able to adjust over time to public E-Verify mandates rather than universal E-Verify mandates. This is likely due to universal E-Verify mandates having larger effects on the population and labor supply of NCI Hispanics than public E-Verify mandates.

A reduction in firm productivity might lead to lower profits or even to firms shutting down. To examine this possibility, I run regressions using state corporate income tax revenue (a proxy for

⁴⁵Even in the SMYP case, the pre-trend flattens out a few years before SMYP takes effect.

Figure 1.4: State GDP per Capita Dynamic Effects



Notes: 2004 - 2015 Bureau of Economic Analysis (BEA) data adjusted to 2015 dollars. The graphs plot the differences in the estimated effects of SMYP, universal E-Verify mandates, and public E-Verify mandates on log real state GDP between affected states and control states relative to a law implementation year of 0. These estimates net out state and year fixed effects.

firm profitability) and the number of establishments in a state by year as outcomes. These results do not strongly suggest that firms are becoming less profitable or closing in response to SMYP or E-Verify mandates.⁴⁶ This suggests that firms might be offsetting losses with lower payrolls, either by having fewer employees or paying them less. It is also possible that profits for firms in industries that are more dependent on undocumented labor could go down, an outcome I cannot observe, even though state corporate income tax revenue does not significantly fall.

1.6.7 Robustness and Other Analysis

This section of the paper briefly goes over several robustness tests and is broken up into two parts. Part one tests the sensitivity of my results to the various choices I've made in defining treatments. Part two describes an assortment of additional robustness exercises.

Robustness to Treatment Specification

Throughout this paper I have made several assumptions about which observations qualify as being treated. These assumptions can be broken down into a few categories. First, because undocumented immigrants cannot be identified in the ACS, I treat all low-skilled, non-US-citizen immigrants of Hispanic origin, excluding those from Cuba or Puerto Rico, as the best proxy for undocumented immigrants. However, a mass majority of undocumented immigrants are from Mexico. Central America has the second highest share. Thus, it may be fair to question whether it is proper to include immigrants from South America or the Caribbean in my definition for NCI Hispanics. To show that this inclusion is not misrepresenting the impact of SMYP or E-Verify mandates, I run specifications on employment and wages, restricting the definition of NCI Hispan-

⁴⁶A full table of these results are available upon request.

ics to only those from Mexico and Central America. Changing the definition of NCI Hispanics has little impact on the point estimates as seen in Table A.9.⁴⁷ Additionally, my results for naturalized Hispanics, US-born Hispanics, and US-born non-Hispanics do not change if I exclude people of Cuban or Puerto Rican descent from these groups.

Another choice I have made throughout the previous analysis is to focus on three types of treatment: SMYP, universal E-Verify mandates, and public E-Verify mandates. It should go without saying, though, that the real world is more complicated. For one, SMYP laws were held up in courts, sometimes permanently, after passage. It might be reasonable to think that states that simply pass SMYP might also induce undocumented immigrants to leave. Table A.10 shows that adding a fourth treatment for the passage of SMYP has no effect on my main results. Furthermore, SMYP passing has no significant impact on either employment or wages. Table A.10 also shows results for a specification that adds a treatment for states that enter into 287(g) agreements with ICE. 287(g) agreements essentially deputize some members of local law enforcement to take custody of unauthorized immigrants, speeding up the deportation process. It seems possible that these agreements could have similar effects to SMYP since both policies operate through law enforcement. However, even after controlling for 287(g) states, the estimated effects of SMYP and E-Verify mandates do not change. Thus, it does not seem that 287(g) agreements are a source of omitted variable bias.

Finally, due to the lack of monthly data in the ACS, I cannot precisely measure which observations are treated in the years that SMYP or E-Verify mandates are implemented. To get around this, I made the simplifying assumption that all observations in the year of implementation,

⁴⁷For the reader's convenience, I present this and all subsequent robustness tables with results for employment and log wages for NCI Hispanics only.

regardless of the month, are treated. There are a variety of different assumptions about the timing of treatment I could have made. In Table A.13, I show the estimated impacts of SMYP and E-Verify mandates under alternate timing assumptions.⁴⁸ Generally, the estimates do not change much.

Additional Robustness Tests

One notable difference between my paper and the previous literature is the broad age range I use. To show that this is not driving my results, I estimate the effects of SMYP and E-Verify mandates under different restrictions on the definition of “working-age.” My estimated coefficients, as seen in Table A.12, are robust to using other common age ranges.

I can also show that neither SMYP nor E-Verify mandates have any noticeable impact on high-skilled workers. This is to be expected, since high-skilled workers are extremely unlikely to be competing directly with undocumented workers for jobs. This also offers additional evidence that the total economy results I find in Section 6.6 are unlikely to be due to a general economic downturn, independent of immigration reform. Such a downturn would almost certainly impact high-skilled individuals as well.

Including observations from Georgia has no effect on my estimated E-Verify effects, but does reduce the estimated impact of SMYP, as would be expected given that the state’s largest population centers have made it known that they will not enforce it. Lastly, my results are robust to removing all state business cycle controls and estimating without using the ACS population

⁴⁸These include treating all observations in partially treated years as being control observations rather than treated, rounding the treatment date to the nearest January 1, and weighing the treatment dummy on observations in partially treated years by the fraction of the year that was actually treated.

weights.

1.7 Conclusion

Using a difference-in-difference framework, I estimated the impact of state immigration reform on a wide variety of labor market outcomes for unauthorized immigrants and their most likely competitors for jobs. The two types of reforms I focus on are SMYP laws and E-Verify mandates. SMYP operates primarily through law enforcement by requiring police to make efforts to check the immigration status of an individual they have reasonable suspicion may be present in the country illegally. Thus, SMYP is likely to encourage unauthorized immigrants to leave the state or take more precaution in public by limiting interactions with law enforcement. This would tend to reduce the labor supply of unauthorized immigrants. It also makes undocumented immigrants riskier for employers to hire, potentially reducing labor demand as well. E-Verify mandates, on the other hand, require employers to submit new employee information so that the legal status of new hires can be confirmed. Thus, E-Verify, while it can lead to unauthorized immigrants leaving the labor force and even the state, will most certainly reduce labor demand for unauthorized immigrants as employers will want to avoid noncompliance penalties. Both SMYP and E-Verify would, in theory, unambiguously reduce the employment of unauthorized immigrants, but could either increase or decrease their wages depending on the magnitudes of the shifts in labor supply and labor demand.

The results support the economic theory and suggest a relatively large decrease in labor demand for undocumented immigrants following immigration reform. Universal E-Verify mandates reduce the employment as a percentage of population of NCI Hispanic (the most likely to be unauthorized immigrants) men by 3.4 percentage points relative to their counterparts in con-

trol states after controlling for individual characteristics, state business cycle indicators, and state, year, and industry fixed effects. Universal E-Verify mandates also reduce wages by 2.7% for these men. These effects are also shown to be long lasting. Public E-Verify mandates and SMYP lead to smaller and shorter-lived, but still significant, reductions the employment and wages of NCI Hispanic men. Furthermore, all three policies result in large decreases in the total population and number of employed NCI Hispanics. These results are all consistent with the notion that SMYP and E-Verify mandates reduce the labor supply of undocumented immigrants. The corresponding wage decreases suggest large decreases in the labor demand for undocumented immigrants.

Evidence also suggests that SMYP and E-Verify operate through different channels. SMYP increases ICE's involvement in states, whereas E-Verify mandates do not. This is most likely due to local law enforcement officers enforcing SMYP and checking the immigration status of undocumented immigrants more frequently. Thus, even policies that are not directly operating through employers or other labor market institutions can still lead to economically significant changes in labor market outcomes for undocumented workers. This idea is further supported by showing that undocumented workers in states with both SMYP and universal E-Verify mandates experience larger decreases in employment and wages than those in states with only universal E-Verify mandates. SMYP can and does affect the labor market independently of E-Verify mandates.

These employment decreases among the targeted population do not seem to lead to increased employment among naturalized Hispanics, US-born Hispanics, or US-born non-Hispanics. This result goes against conventional thinking and suggests that low-skilled US citizens either are not strong labor substitutes for undocumented workers on average or are simply unwilling to work the same jobs. Given this result, it is perhaps not surprising that policies that induce undocumented immigrants to leave a state have a net-negative impact on that state's overall economy. In particular,

universal E-Verify mandates reduce state GDP per capita by 3.1% and hit the construction industry particularly hard. SMYP, conversely, reduces state agriculture GDP per capita significantly.

To give these numbers more context, consider that the average per capita GDP in the year prior to universal E-Verify mandates taking effect in states that have implemented them was about \$40,252. Thus, universal E-Verify mandates reduced state GDP per capita by \$1,248 on average. Scaling this number up by the mean population in these states over the same years shows that universal E-Verify mandates reduced total state GDP by an average of \$8.12 billion. In states that implement both SMYP and universal E-Verify mandates, these mean productivity declines increase to \$2,040 per capita and \$12.07 billion in total.

What does this analysis mean for the future of immigration policy in the US? First, policy-makers should carefully consider the potential macroeconomic effects of removing unauthorized immigrants in addition to how low-skilled labor markets will be affected. Even if my estimates of the productivity costs of removing undocumented immigrants are only in the ballpark of the true values, they would still be quite large and economically significant. Furthermore, these estimates do not even attempt to account for any costs associated with actually finding, holding, and deporting undocumented immigrants. Second, one of the major supposed benefits of removing undocumented workers are the employment gains for US workers. These gains, however, are not being realized, and given the large costs of immigration reform, there are perhaps more effective and efficient ways of achieving better employment opportunities for low-skilled Americans.

Chapter 2

Drunken Mistakes: Testing for Heterogeneity in Tax Salience Responses in Alcohol Consumption

2.1 Introduction

In standard economic tax theory, a key assumption is that consumers perfectly adjust their behavior after a tax change. Several important policy implications follow directly from this. For example, Ramsey's optimal taxation theory relies on this assumption of perfect behavioral adjustments to derive the efficient tax rates that maximize social welfare. Furthermore, standard tax theory assumes a homogenous response to taxes despite heterogeneity among consumers. It is possible, and perhaps even likely, that many of our current tax policies are based in some part on this assumption. Unsurprisingly, there could be major cause for concern from both an efficiency and equity standpoint if this assumption is violated.

The basic premise behind consumer tax salience is that consumers may not be perfectly responding to taxes. Consumers may be unaware when taxes change or even of what the current tax rates are. Even if consumers are fully aware of the current tax code, they may still not adjust their behavior perfectly. For example, consumers may under-respond to increases in sales taxes even if they know the relevant sales tax rates. One possible explanation of this phenomenon is that some type of cognitive cost exists. It may be more mentally taxing for a consumer to calculate how much they will owe at the register for a sales tax, so he or she may make rough estimates of

the after-tax price instead. In general, salience theory predicts that consumers respond less to taxes that are less salient, such as sales taxes that are added at the register, than to taxes that are more salient, such as excise taxes, which are included in the sticker price of goods.

If consumers are not perfectly salient, there are several potential policy implications. First, if consumers are not altering their behavior by as much as standard theory predicts, then the dead-weight losses associated with tax policies are less than the theory predicts. Additionally, the tax revenue raised by the government increases as consumer salience decreases. These two results have an impact on overall social welfare and will be discussed in more detail in Section 2. Second, if consumers are heterogeneous in their saliency, there could be major implications on equity. Tax policies are often described in terms of being progressive or regressive. It would be very concerning indeed if poor consumers are less salient than rich consumers. This would imply that the poor are altering their behavior less as taxes change, and thus face an increasing share of the tax burden, either exacerbating regressive taxes or mitigating some of the effects of progressive taxes.

In this paper, I examine the question of whether or not consumers under-respond to less salient taxes, such as the sales tax. I also look for possible heterogeneity in these responses across several dimensions of observable characteristics including income, education, and age. I look at this question in the context of the market for alcohol. This is an interesting setting to test tax salience theory since alcohol is subject to both a per-unit excise tax and a sales tax in the United States. The excise tax is included in the sticker price, making it very apparent to consumers how it affects the price. The sales tax is not added until consumers pay at the register, making it less salient to consumers. Alcohol is also frequently subject to policies that attempt to decrease its consumption, such as the excise tax. Despite this, millions of Americans continue to consume alcohol. This makes alcohol a publicly important good worth studying.

The economic literature on tax salience has been growing recently. In general, these papers find evidence of tax salience effects across a wide array of different settings.¹ As far as tax salience with respect to the sales tax is concerned, there are two seminal papers.

The first of these papers is Chetty, Looney, and Kroft (CLK) (2009). CLK provide two empirical tests of sales tax salience theory. First, they run a controlled experiment in California grocery stores and find that demand is significantly reduced when consumers see the after-tax price posted on items. Of greater importance to my study, they examine how state-level aggregate sales of alcohol vary with changes in excise taxes versus changes in sales taxes. Their results suggest that state-level increases in excise taxes decrease alcohol consumption significantly more than similar increases in sales taxes. My paper builds off of this setting but has the advantage of being able to observe individual level alcohol consumption. This allows me to test for heterogeneous responses among groups of alcohol consumers to changes in taxes.

Goldin and Homonoff (2013) build off of CLK by testing tax salience theory in the context of individual demand for cigarettes. They also test for heterogeneous tax salience effects across consumers with different incomes. Their results show that low-income consumers respond more to sales tax increases than high-income consumers, suggesting that low-income consumers are more tax salient. My paper builds off of their approach in the context of alcohol demand.

To understand how my paper provides a contribution to the existing literature, it is important to understand some key differences between alcohol consumers and cigarette consumers.

¹For instance, Finkelstein (2009) shows that toll roads that adopt electronic toll collection methods are able to charge significantly higher rates to drivers. Cabral and Hoxby (2013) show that in areas with higher usage of tax escrow (a less salient method of paying property taxes), property tax rates are higher and property tax revolts are less likely.

More people in the United States drink alcohol than smoke cigarettes.² This means that the sample of drinkers is likely to be more similar demographically to the true population of the United States than the sample of smokers. This, combined with the fact that alcohol policies, including the excise tax, are often implemented with the explicit purpose of reducing alcohol consumption, shows that alcohol is a good of great public importance. My paper serves as a test of salience theory in the market for this socially relevant good. Any information on consumer responses to the taxation of alcohol would be of great importance to policymakers.

Another major contribution to the literature in this analysis is that I test for heterogeneity in tax salience across other dimensions than solely income. Since factors such as age and education are highly correlated with income, it could be that the differences in salience that arise due to income are in fact due to other factors. Heterogeneity in education is particularly interesting to examine since it may provide some insight into potential mechanisms for tax salience. I also examine heterogeneous tax salience responses with respect to age, marital status, race, and sex. Finally, my paper serves as a check of the results of both CLK (2009) and Goldin and Homonoff (2013).

To test salience theory, I utilize the Behavioral Risk Factor Surveillance System (BRFSS) dataset to identify the effects on individual-level alcohol consumption of plausibly exogenous state changes in alcohol excise and sales tax rates for the years 1984-2003. I find suggestive evidence supporting consumer tax salience theory. Namely, I find that consumers respond significantly to changes in excise tax rates with a demand elasticity of about -1.404 and insignificantly to changes

²According to the Center for Disease Control and Prevention (CDC), roughly 18.1% of adults in the United States currently smoke. In contrast, the CDC reports that 51.3% of adults consume at least 12 drinks per year, and another 12.9% of adults consume between 1 and 11 drinks per year in the United States.

in sales tax rates. However, there is little evidence for heterogeneity in consumer tax salience across income groups. Instead, I show that consumers with higher education are significantly more tax salient than consumers with low education. This may be seen as supporting the hypothesis that there is a cognitive cost to accounting for after-tax prices.

The paper is organized as follows. Section 2 sets up a basic model explaining tax salience theory and contrasts it with the standard neoclassical model of tax theory. Section 3 provides the relevant details of alcohol consumption and taxation in the United States. Section 4 discusses the empirical methodology and identification strategies used in this paper. Section 5 discusses the data sources. Section 6 presents my empirical results, including testing for heterogeneity in tax salience responses. Finally, Section 7 discusses the possible implications of these results.

2.2 Salience Theory

The basic premise behind tax salience theory is that taxpayers do not respond “perfectly” to changes in tax rates. That is, taxpayers empirically do not behave in the same way that the neoclassical model predicts. To see the implications of this model, consider a simple model of supply and demand with taxation.³

For expositional simplicity suppose that demand in the presence of taxation is given by the following estimating equation:

$$\log[x(p, \tau^e, \tau^s)] = \alpha + \beta \log[(p + \tau^e)(1 + \tau^s)]$$

³This model, specifically the demand equations, is based off of the model discussed in CLK (2009). I use this setting since it is most relevant for the following analysis, but a similar extension can be made to labor-leisure models and intertemporal consumption models among others.

where $x(p, \tau^e, \tau^s)$ is the demand for good x as a function of the pre-tax price p , wholesale excise tax τ^e , and sales tax rate τ^s . Let θ_τ be a term that captures consumer salience to the sales tax. In CLK (2009), θ_τ is defined as the demand elasticity with respect to the tax rate on good x relative to the demand elasticity with respect to the price of good x . That is, θ_τ is the percent change in demand for good x with respect to the percent change in sales tax rate τ^s divided by the percent change in demand for good x with respect to the percent change in the sticker price $p + \tau^e$ of good x .⁴ Consumer salience enters the model in the following way:

$$\log[x(p, \tau^e, \tau^s)] = \alpha + \beta \log[(p + \tau^e)] + \theta_\tau \beta \log[(1 + \tau^s)]$$

Here θ_τ measures the differential consumer response to changes in the sales tax relative to changes in price (or excise tax).⁵ Though in theory, θ_τ need not be constrained, tax salience literature focuses specifically on the case where $0 < \theta_\tau < 1$. This is exactly the case of consumers under-responding to changes in sales tax rates, which has been most frequently observed in the empirical literature. In this case, the response to changes in the sales tax rate is exactly the fraction θ_τ of the response to changes in the sticker price.

With salience effects, consumers do not alter their behavior as much in response to changes in tax rates relative to the neoclassical model of taxation. This leads to two important policy implications. First, the deadweight loss of the tax is potentially much lower in the presence of salience effects. This could have large implications on the efficiency of tax policies as larger tax rates are seemingly more efficient in the presence of salience effects. Second, tax revenue is larger with salience effects. For each level of tax, the quantity transacted increases, thus increasing revenue.⁶

⁴I have made the assumption that the excise tax is included in the posted price of good x and the sales tax is not. This is the case in almost every state for alcohol.

⁵Indeed changes in the excise tax predict changes in the price of alcohol remarkably well. See CLK (2009).

⁶The interested reader can find a quick visual demonstration of these implications in the appendix.

This can have numerous effects on government policy. For example, if a government takes account of salience effects, the optimal tax rate they choose to achieve a set revenue requirement could possibly be lower.

Even knowing that in the presence of salience effects the amount of deadweight loss unequivocally falls and the amount of tax revenue unequivocally rises for a given tax policy, it is difficult to extrapolate this theory to aggregate social welfare without making any assumptions about consumer utility functions and the uses of government revenue. These assumptions are beyond the scope of this analysis. This paper is primarily concerned with estimating the elasticities that can be used to calculate θ_τ for heterogeneous groups of consumers. I will now discuss some other relevant features of the alcohol consumption market in the United States.

2.3 Setting

I examine individual alcohol consumption in the United States from the period of 1984–2003. This setting is conducive to testing for tax salience effects for a number of reasons. Many of these have been mentioned above, but I will highlight some of the key features of this market here.

Alcohol is subject to two forms of consumption taxation in the United States. The first of these is an excise tax that is levied at both the federal and state government levels. These taxes are typically applied as a fixed dollar amount per gallon and placed on firms at the wholesale level. This directly affects the price that retailers pay for alcohol, and by extension, affects the price that consumers pay for alcohol. In particular, the tax is included in the sticker price that consumers see when they purchase alcohol. As such, any changes in the excise tax levied on alcohol would show up to consumers as a change in price regardless of whether or not consumers actually know the

excise tax has changed.⁷

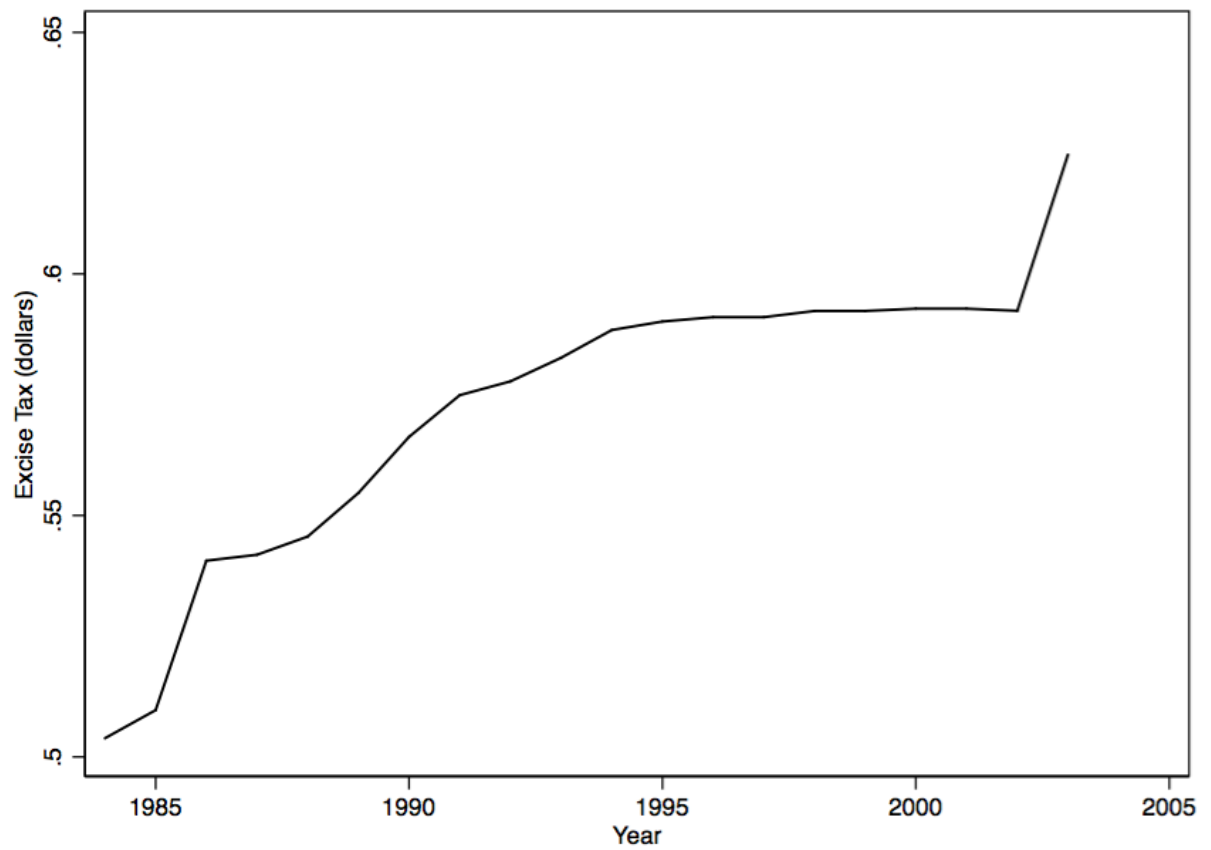
In addition to the excise tax, alcohol is also subject to a sales tax in most states.⁸ In most cases, alcohol is subject to the same sales tax rate as general goods, but a few states impose a higher sales tax rate on alcohol than other goods. Unlike excise taxes, which are levied in dollars per gallon, all the sales taxes are levied as a percent of the price of alcohol. Furthermore, in nearly every state, sales taxes are not added to the price until the consumer checks out at the register. Thus, the final price a consumer pays for alcohol is actually higher than the sticker price the consumer sees. Under standard salience theory, consumers would be expected to respond more to changes to the excise tax than changes to the sales tax. This is because the excise tax is included in the sticker price, whereas the sales tax is not. The excise tax is more salient than the sales tax.

Of great importance to this study is the fact that there is ample variation in both the sales and excise tax rates across both states and time. Figures 2.1 and 2.2 show the average annual excise taxes and sales tax rates applied to beer from 1984–2003. Both taxes have been increasing over time, though the nonlinear pattern of the lines suggest that the increases have been of different magnitudes and across different numbers of states in each year. All in all, between 1984 and 2003, there were a total of 55 changes in state beer excise taxes and 63 changes in state sales taxes. These were distributed across 25 and 33 states respectively. Provided these changes in tax rates are exogenous to alcohol consumption patterns, this will provide the necessary variation identify the

⁷This, of course, relies on retailers and wholesalers being aware of changes in the excise tax rate. Wholesalers are very likely to know excise tax rates since they actually pay the tax. Given that changes in alcohol retail prices are highly correlated with changes in excise taxes, it seems very likely that retailers are also aware or at the very least, seeing the excise tax passed through by wholesalers via higher wholesale prices. CLK (2009) are able to provide evidence that the passthrough of the excise tax from retailers to consumers is close to 100% using proprietary and restricted pricing data.

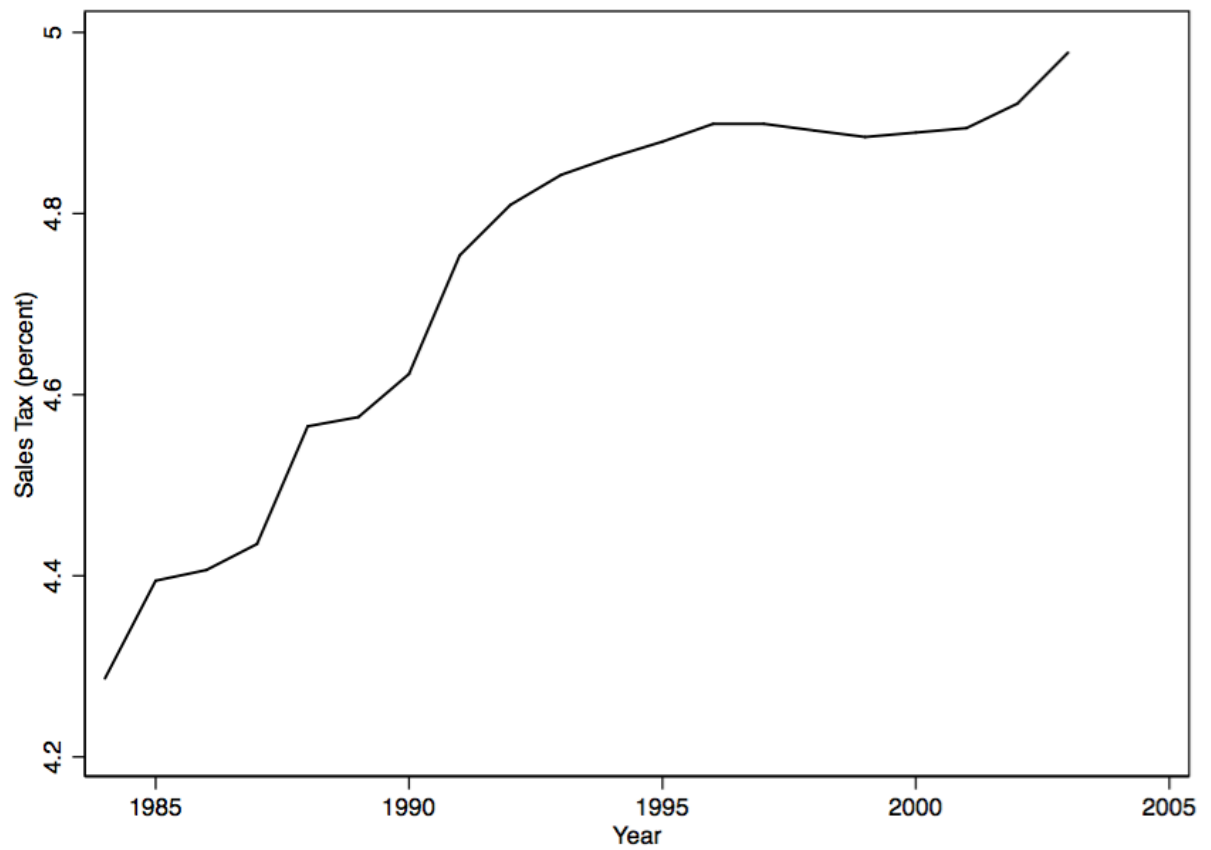
⁸As of 2003, only five states did not have a sales tax that applied to alcohol: Alaska, Delaware, Montana, New Hampshire, and Oregon.

Figure 2.1: Average State Excise Tax



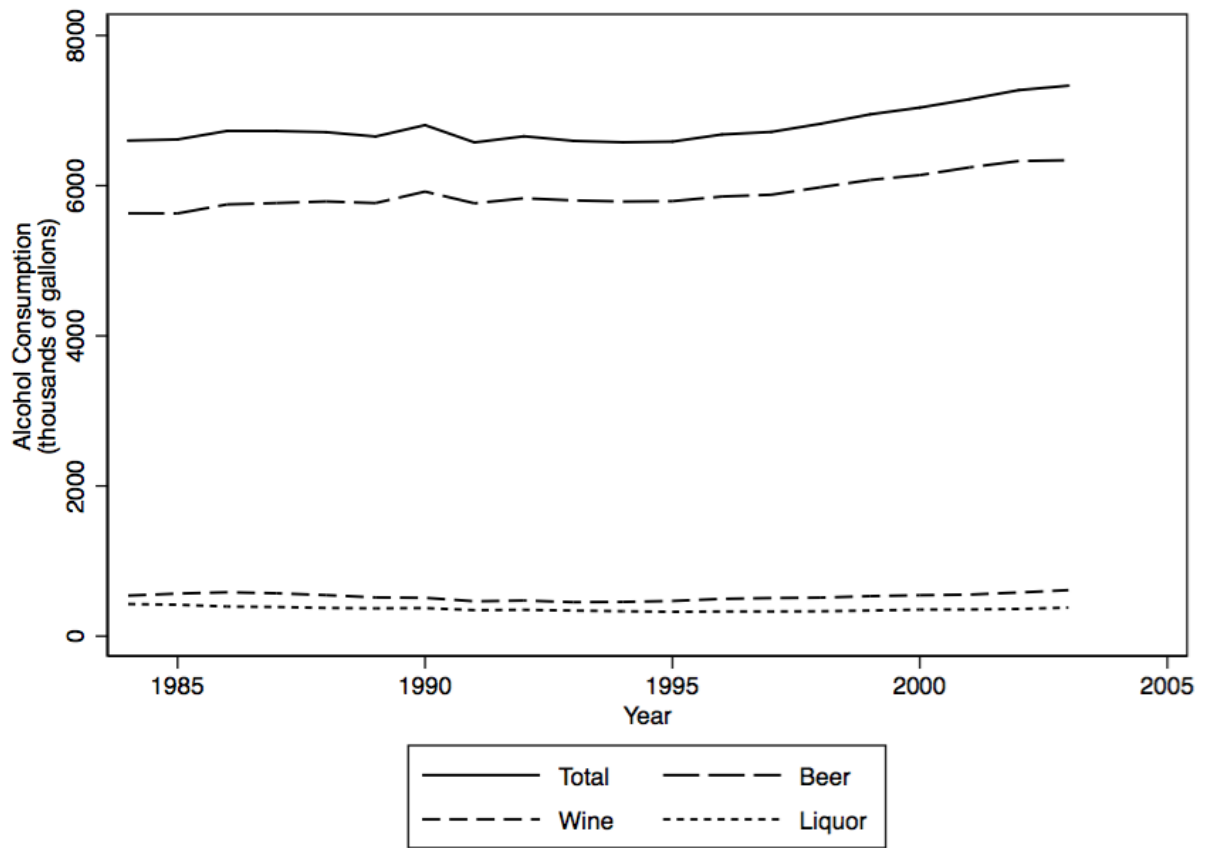
This figure shows the average state excise tax measured in dollars per gallon of beer over the years 1984-2003.

Figure 2.2: Average State Sales Tax



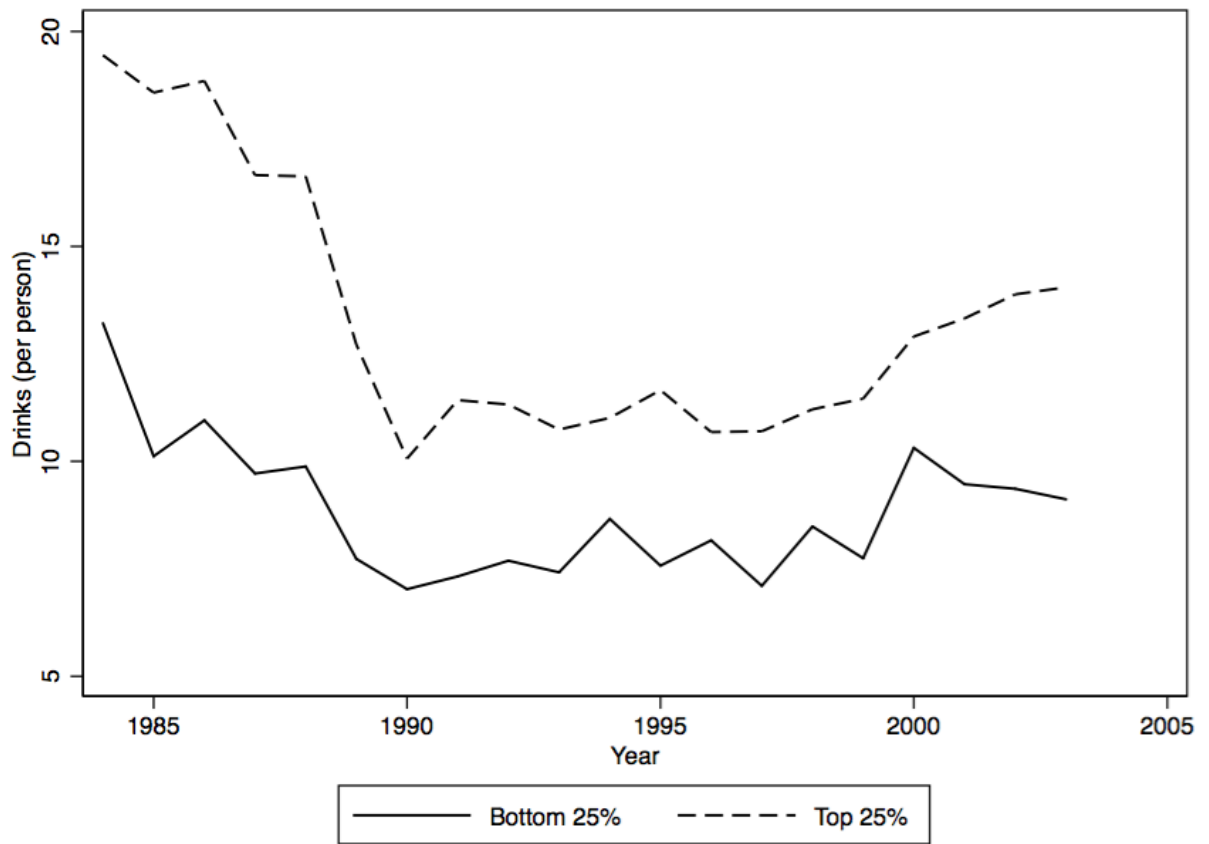
This figure shows the average state sales tax rate applied to beer over the years 1984-2003.

Figure 2.3: Aggregate Alcohol Consumption



This figure, based off of alcohol consumption data collected by CLK (2009), shows the average state-aggregate consumption of different classes of alcohol over the years 1984-2003.

Figure 2.4: Average Drinks Consumed by Income



This figure shows the average number of drinks per person consumed by the bottom 25% of the income distribution and the top 25% of the income distribution over the years 1984-2003.

salience effects of these taxes on consumer decisions. I discuss this assumption in more detail in Section 4.

A couple of other details about alcohol consumption in the United States are worth noting. First, as seen in Figure 2.3, aggregate alcohol consumption has trended slightly upward since 1984. This suggests that controlling for time trends will be important for measuring the true consumption responses to sales and excise taxes. Figure 2.3 also shows that beer makes up the vast majority of alcohol consumption in America. Furthermore, beer consumption seems to follow the general trend of aggregate alcohol consumption, while wine and liquor consumption seem to be relatively flat across time.

Second, individual alcohol consumption differs across income levels. Figure 2.4 shows that consumers in the bottom quartile of the income distribution drink less than consumers in top quartile. Furthermore, these two groups of consumers do not seem to necessarily follow the same trend in alcohol consumption over time.⁹ This suggests that they may have heterogeneous alcohol consumption behaviors and possibly heterogeneous tax salience responses. This possible heterogeneity in tax salience, in addition to other possible sources of heterogeneity, will be examined in Section 6.2.

⁹A graph showing consumption trends for the middle two quartiles shows similar results, though consumers in these quartiles seem more similar to consumers in the top quartile.

2.4 Methodology

My empirical model follows as a direct extension of CLK (2009) and Goldin and Homonoff (2013). The baseline specification is:

$$y_{ismt} = \alpha + \beta_1 \tau_{smt}^e + \beta_2 \tau_{smt}^s + \gamma x_{smt} + \delta z_{ismt} + \mu_s + \mu_m + \mu_t + \mu_{st} + \varepsilon_{ismt} \quad (2.1)$$

where y_{ismt} is the log alcohol consumed by person i in state s , calendar month m , and year t . τ^e is the log excise tax rate, and τ^s is the log sales tax rate. x represents covariates that do not vary among individuals surveyed in the same state, year, and month, including log average state income and log state unemployment levels. z represents covariates that do vary among individuals, such as gender, race, education, and income. The model also includes state, year, and month fixed effects as well as state-specific linear time trends to capture any seasonal and time patterns in alcohol consumption. My main specification and all subsequent specifications rely on using survey weights for each observation and clustering standard errors at the state level.¹⁰

The coefficients of interest are β_1 and β_2 which can be thought of as the estimated demand elasticities of alcohol with respect to the excise tax rate and sales tax rate, respectively. For there to be evidence of tax salience theory, consumers would have to reduce consumption more when the excise tax increase than the sales tax, that is $\beta_1 - \beta_2$ should be less than zero. This term, $\beta_1 - \beta_2$, is referred to in the prior literature as the “attention gap.” Using my estimates of these elasticities, I can estimate the consumer under-response parameter θ_τ as well. In this case, θ_τ can be calculated as $\frac{\beta_2}{\beta_1}$.

¹⁰The survey weights are used to counteract the fact that the BRFSS is not nationally representative, especially in its earlier years.

One additional aspect of this model that is worth mentioning is that τ^e is measured as an excise tax rate, whereas in practice, the excise tax is implemented as a fixed dollar amount per quantity. In order to convert the excise tax into an ad valorem percentage tax, and thus make it comparable to the sales tax, I follow the technique specified by CLK (2009). I divide the excise tax for a case of beer in year 2000 dollars by the national average price of a case of beer in 2000.¹¹ The average US price is chosen instead of state-level prices since the excise tax is endogenous to a state's price. Relaxing this restriction by allowing for regional average prices instead of the national average price has little effect on the results.

Identification in this model derives from the arguably exogenous changes in both excise and sales tax rates. Identification also relies on the different years of data being comparable to one another. Issues pertaining to these assumptions will be discussed in Section 5.

To test for heterogeneity among consumers, I use the following extension of equation (2.1):

$$y_{ismt} = \alpha + \beta_1 \tau_{smt}^e + \beta_2 \tau_{smt}^s + \rho_1 \tau_{smt}^e K_{ismt} + \rho_2 \tau_{smt}^s K_{ismt} + \eta K_{ismt} + \gamma x_{smt} + \delta z_{ismt} + \mu_s + \mu_t + \mu_m + \mu_{st} + \varepsilon_{ismt} \quad (2.2)$$

where K is an indicator of if individual i in state s , year t , and month m belongs to the relevant group.¹² I first focus on heterogeneity across income groups, where K is an indicator of if a consumer has an income in the bottom 25% of the state-year income distribution, in order to draw comparisons with Goldin and Homonoff (2013). Other dimensions I test for heterogeneous tax responses across include high school graduates, college graduates, female consumers, white

¹¹A case of beer is the equivalent of twenty-four 12 ounce cans or bottles.

¹²By “group”, I mean the individuals with the characteristic over which I am testing for heterogeneity in tax responses. For instance in looking at marital status, K would be a dummy variable that equals one if the individual is married and zero if not.

consumers, married consumers, young consumers (defined as being younger than 25 years old), and old consumers (defined as being older than 64).

In this specification, β_1 and β_2 represent the first group of consumers', say high-income consumers', responses to changes in the excise tax and sales tax, respectively. In this example, low-income consumers' elasticity of demand with respect to the excise tax is measured by $\beta_1 + \rho_1$ and by $\beta_2 + \rho_2$ for the elasticity of demand with respect to the sales tax. While these values and the resulting attention gaps of these two groups of consumers are policy-relevant on their own, it is arguably more interesting to consider the differences in salience responses across these two groups of consumers. In the model, this difference is estimated as being $\rho_2 - \rho_1$.¹³ In this example, if $\rho_2 - \rho_1$ is less than zero, then low-income consumers pay more attention to changes in the sales tax than high-income consumers.

2.5 Data

For my primary source of data on alcohol consumption, I utilize the Behavioral Risk Factor Surveillance System (BRFSS), a cross-sectional survey containing detailed information on several health indicators. The survey has been run by the National Center for Chronic Disease Prevention and Health Promotion and the Centers for Disease Control and Prevention from its inception in 1984. Individual adults are surveyed over the telephone and answer questions related to their health measures, behaviors affecting health, and individual characteristics.¹⁴ I choose to restrict

¹³This is derived by taking the difference in the attention gaps of the two types of consumers: $[(\beta_1 + \rho_1) - (\beta_2 + \rho_2)] - [\beta_1 - \beta_2] = \rho_2 - \rho_1$.

¹⁴Initially the survey was conducted only using landlines, but has since expanded to include cell phones as well. Even so, demographics that are less likely to have phones are more likely to be underrepresented. To get around this issue of not being a representative sample, the BRFSS provides a series of population weights to better match its cross-section of data to the entire US population.

my analysis to the years of 1984–2003 to make my estimates more comparable to CLK (2009) and Goldin and Hominoff (2013). I also restrict the majority of my analysis to focus on individuals age 21 or older. By 1984, most states had implemented a legal drinking age of 21.¹⁵

There are many advantages of the BRFSS dataset. Primarily, it matches consumers' individual characteristics to their alcohol consumption. These characteristics include state of residence, age, education level, marital status, income level, number of children, and a rich array of health and behavioral indicators. This allows for the ability to make comparisons of any changes in demand for alcohol in response to different tax changes across heterogeneous groups of consumers. The BRFSS also contains over 1.5 million observations in my sample period.

There are a few measures of alcohol consumption in the BRFSS dataset. The first is a binary measure of if the respondent has had any alcohol to drink in the past month. The second measure asks consumers how many days they had a drink in the past month, and the third asks for the average number of drinks they had on the days they did consume alcohol.¹⁶ Using these responses, the BRFSS constructs a measure of the number of drinks each individual consumes per month. In order to estimate the tax elasticities of demand, I need to take the natural log of this measure of total alcohol consumption. Since I want to account for the fact that a proportion of the population does not drink and that this decision may be impacted by tax rates, prior to taking the natural log of alcohol consumption, I add one to each observation. This preserves any trends in the data and provides estimates that are roughly equivalent to the true sample effects of tax changes on total alcohol consumption.

¹⁵Including consumer between the ages of 18 and 20 has little effect on my results.

¹⁶According to the BRFSS Codebook, an interviewee is informed that a drink is equivalent to “one can or bottle of beer, one glass or wine, one can or bottle of wine cooler, one cocktail, or one shot of liquor.”

There are a couple of drawbacks to the BRFSS data. Unfortunately, in most years of the survey there are no questions about the types of alcohol consumed.¹⁷ Thus, I make the simplifying assumption that all alcohol consumed is beer, and by virtue subject to beer taxes. As Figure 2.3 shows, beer consumption makes up the vast majority of alcohol consumption over this time period and follows the same trends as total alcohol consumption. Additionally, CLK (2009) find that beer tax rates are highly correlated with both wine tax rates and liquor tax rates.¹⁸ This suggests that my assumption that beer consumption is a good proxy for alcohol consumption will not severely bias my results.

Another issue is that the survey only had 15 participating states in 1984. This grew to all 50 states by 1994. To confound this issue, starting in 1994, a majority of states only ask alcohol related questions every other year. This potentially adds extra noise to the measures of alcohol consumption. Thus, my estimates might not be as precise as could be ideally hoped for with such a large sample size. The income variable in the BRFSS also cause some potential problems. Income is measured in bins and top-coded at only \$75,000. Additionally, the income measure does not adjust for changing price levels. To account for this, I generate a distribution of incomes by both state and year by assigning the midpoint percentage of observations in an income category to each observation.¹⁹

Data on state beer excise tax and sales tax rates come from the Brewer's Almanac, World Tax database, and other sources as specified by CLK (2009).²⁰ All excise taxes and prices are

¹⁷Questions relating to beer per month, wine per month, and liquor per month were only asked from 1984 through 1988.

¹⁸They find that beer taxes have correlation coefficients of around 0.94 with other alcohol taxes and that 86% of the time that beer taxes change, taxes on other alcohol change as well.

¹⁹This follows directly from the methodology of Goldin and Homonoff (2013) and Franks, et al. (2007).

²⁰The actual data they use is available courtesy of the American Economic Association on their website for any

converted to year 2000 dollars. As mentioned earlier, excise taxes are converted to tax rates by dividing by the average national price of beer in the year 2000. Finally, observations from Hawaii and West Virginia are dropped for the purposes of this analysis. This is due to Hawaii including sales tax in the sticker price of alcohol and West Virginia changing its sales tax base frequently over this time period.

For the model to be identified, changes in both taxes need to be exogenous. It is unlikely that sales taxes are correlated with the error term since sales taxes generally affect many more goods than just alcohol, but it is possible that excise taxes are. Excise taxes are targeted at specific goods and do not directly affect the prices of other goods. This allows excise taxes to be used to discourage the consumption of goods that society associates with negative externalities. If a state were to have a large number of drunk driving accidents, for instance, then there may be a push for higher taxes on alcohol to try to discourage drinking. A robustness check where I add in controls for various other state alcohol policy changes that might be accompanied by increases in the excise tax has little impact on my results, alleviating some of this concern.²¹

The model assumes that changes in the excise tax are equivalent to changes in the price of alcohol. While, strictly speaking, this is true, there could be issues if sellers adjust the price of alcohol in response to an increase in taxes beyond simply accounting for the changes in taxes. In particular, it is possible that alcohol sellers are themselves not salient to tax changes and “under” or “over” adjust their prices. This would lead me to misinterpret my results as evidence of consumer tax salience when in fact, they could simply be responding to price changes by producers who are

interested readers.

²¹CLK (2009) also provide some additional robustness checks to support the assumption that excise taxes are plausibly exogenous.

not behaving optimally. Though, I do not directly observe the price of alcohol, I run robustness checks using regional estimated prices of six-packs of beer as a proxy for the price of alcohol. Including the price has little effect on my results.

Figures 2.5 and 2.6 show the raw trend relationship of the log total number of drinks per month consumed by an individual (including zero) to the log excise tax rate and the log sales tax rate, respectively. Both figures show that as the taxes increase, the number of drinks decreases. However, the downward trend is much stronger in response to increases in the excise tax than the sales tax. This suggests that individual consumers are, in fact, responding more negatively to excise taxes than sales taxes just as salience theory predicts. Adding in a richer set of covariates will show a clearer picture of the magnitudes of these different responses.

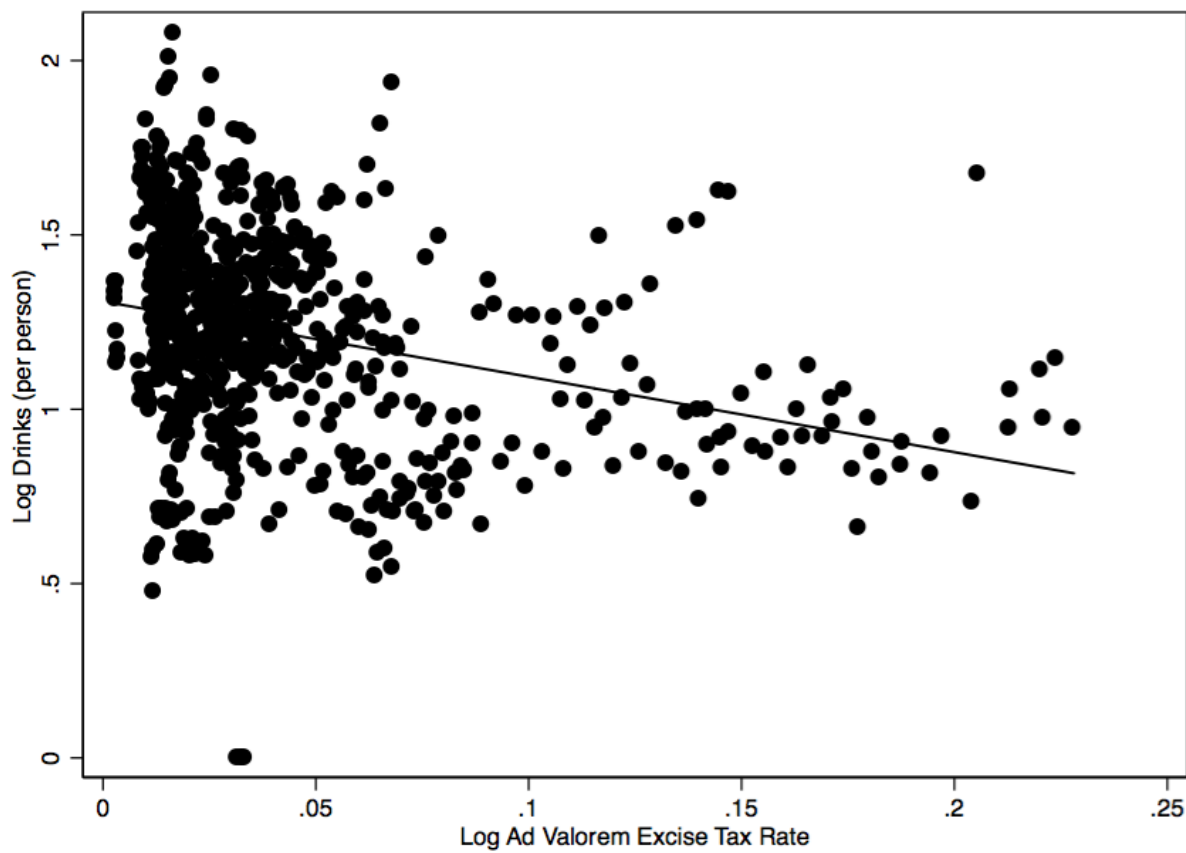
2.6 Results

2.6.1 Baseline Results

Table 2.1 presents the results from estimating equation (2.1). The outcome of interest is an individual's log total number of drinks per month.²² Column (1) shows estimates based off a specification that controls for individual characteristics and month, year, and state fixed effects in addition to state-specific linear time trends. Column (2) adds in log monthly state unemployment rates and log quarterly state per capita income. Column (3) adds in income-specific time trends to allow for the possibility that low-income consumers might have different trends in alcohol consumption. All observations are weighted using the BRFSS survey weights, and all standard errors are clustered at the state level. The individual covariate and sales tax coefficients are robust to not

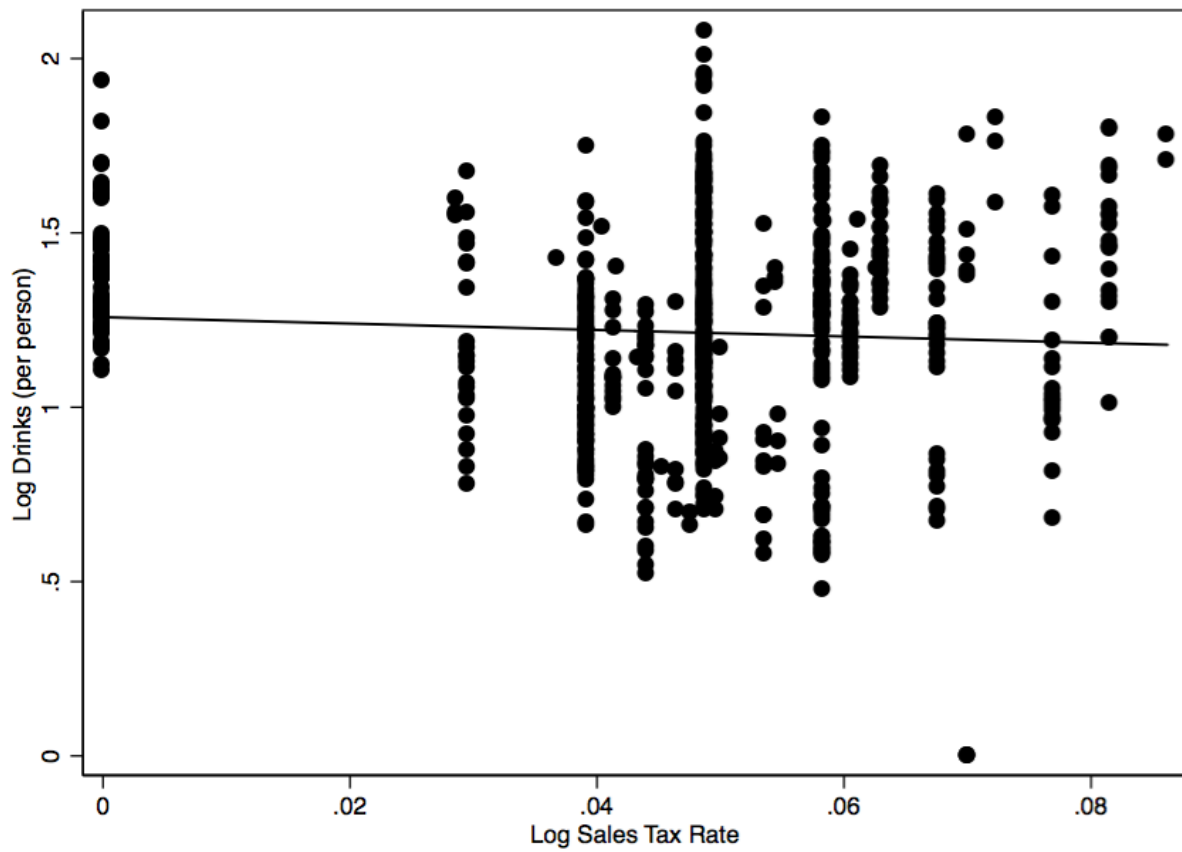
²²As mentioned previously, this is the natural log of the total number of drinks the BRFSS calculates that an individual drank in the past month plus one to account for individuals who had zero drinks.

Figure 2.5: Individual Alcohol Consumption and State Beer Excise Taxes



This figure plots the average log individual alcohol consumption as measured in drinks against the average $\ln(1 + \tau^e)$, where τ^e represents the calculated ad valorem excise tax rate applied to beer. Each point represents the average drinks consumed in a state-year cell.

Figure 2.6: Individual Alcohol Consumption and State Sales Taxes



This figure plots the average log individual alcohol consumption as measured in drinks against the average $\ln(1 + \tau^s)$, where τ^s represents the sales tax rate applied to beer. Each point represents the average drinks consumed in a state-year cell.

including income trends or state economic variables. The excise tax coefficient, however, is not. In particular, controlling for state economic conditions seems to have a large effect on the point estimate. These could possibly be capturing other factors that, like the excise tax, directly impact the price of beer. As such, I consider the model including state economic controls and income trends as my preferred specification when presenting results for the remainder of this paper.

The results show that individuals are reduce alcohol consumption in response to increases in both excise and sales taxes as is expected. Furthermore, at first glance, consumers seem to have much stronger responses to changes in the excise tax than to changes in the sales tax. In columns (2) and (3), the excise tax (price) elasticity of demand is significantly negative and nearly twice as large in magnitude as the insignificant sales tax elasticity of demand. Using these estimated elasticities, I can estimate the average salience for individuals in this market θ_τ by dividing -0.794 by -1.404. This implies that θ_τ is roughly 0.57.²³

However, these results should be treated with some caution. When taking standard errors into account, it becomes obvious that the sales tax elasticity is very imprecisely estimated. This is likely a product of the BRFSS rotating alcohol related questions in different states every other year, greatly reducing the variation among sales tax rate changes I have observable consumption data for.²⁴ These large standard errors lead to me being unable to reject the null hypothesis that the two elasticities are equal. Even so, these results can be seen as suggestive of tax salience effects with regards to individual alcohol consumption.

²³For reference, CLK (2009) estimate that the excise tax elasticity of demand is -0.71, the sales tax elasticity of demand is -0.05, and θ_τ is 0.06. These estimates are for aggregate alcohol consumption and do not control for individual characteristics.

²⁴This is less of a concern for the excise tax rate since by construction, the excise tax rate changes every year due to changing prices, even if the actual dollar amount of the excise tax does not change.

Table 2.1: Effect of Taxes on Alcohol Demand

	(1)	(2)	(3)
Excise tax	-0.704 (0.661)	-1.394** (0.589)	-1.404** (0.592)
Sales tax	-0.879 (2.362)	-0.808 (2.410)	-0.794 (2.401)
Income	0.764*** (0.019)	0.764*** (0.019)	0.698*** (0.034)
Female	-0.658*** (0.012)	-0.658*** (0.012)	-0.658*** (0.012)
Pregnant	-0.924*** (0.031)	-0.923*** (0.031)	-0.923*** (0.031)
White	0.352*** (0.055)	0.353*** (0.055)	0.354*** (0.056)
High school grad	0.161*** (0.010)	0.161*** (0.010)	0.162*** (0.011)
College grad	0.084*** (0.014)	0.084*** (0.014)	0.083*** (0.014)
Married	-0.352*** (0.017)	-0.352*** (0.017)	-0.352*** (0.017)
Unemployed	0.104*** (0.014)	0.103*** (0.014)	0.102*** (0.014)
Age	-0.137*** (0.015)	-0.137*** (0.015)	-0.136*** (0.015)
Economic conditions		X	X
Income trends			X
H ₀ : Excise = Sales			
<i>p</i> -value	0.947	0.828	0.820
Observations	1,455,129	1,455,129	1,455,129

Notes: 1984 - 2003 Behavioral Risk Factor Surveillance System data. All prices and taxes adjusted to 2000 dollars. Standard errors are clustered at the state level in parentheses. All specifications are weighted and include individual demographic characteristics and state, year, and calendar month fixed effects. State-specific linear time trends are also included. Economic conditions include log quarterly state income and log monthly unemployment trends. Income trend is income multiplied by the number of years since 1984. Second-, third-, and fourth-order age polynomials are included in the regression but not displayed. Outcome variable: log total number of drinks consumed by an individual in the previous month (including 0 if the individual reported not drinking any) plus 1. Excise tax refers to $\ln(1 + \text{ad valorem excise tax rate})$. Sales tax refers to $\ln(1 + \text{sales tax rate})$. The *p*-values are for the test of equality in the coefficients specified by H₀.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

2.6.2 Heterogeneous Salience Responses

One possibility that the results in Table 2.1 do not account for is that different consumers might have different tax-awareness levels and different responses. Thus, even if on average, there is only suggestive evidence of tax salience, there may be well defined differences in salience levels across different types of consumers. I test for these differences using estimating equation (2.2). The first dimension of heterogeneity I exploit is income, much as Goldin and Homonoff (2013) do. I consider the possibility that consumers in the bottom quartile of the income distribution might pay more or less attention to changes in the sales tax relative to their wealthier counterparts. Any increase in the sales tax will have a larger effect on poorer consumers budgets, possibly leading them to have stronger consumption responses to the sales tax. On the other hand, poorer consumers might be less aware of tax changes in general, and have smaller responses to changes in the sales tax than wealthier consumers.

In addition to examining differential salience response across income, I contribute to the literature by testing for salience heterogeneity across several new dimensions. First, I consider heterogeneity across education. For this I utilize two groupings. I start by comparing the responses of consumers with at least a high school education to those without one. Then, I compare the responses of consumers with a college degree to those who never completed college. I suspect that having more education will lead to more tax salience in consumers. In order for consumers to accurately take full account of the sales tax, they must be able to compute their after-tax price correctly. This may take time and mental capacity that many consumers are either unwilling or unable to spend. Having a higher level of education arguably makes these cognitive costs lower and facilitates accounting for the sales tax. I next examine heterogeneity across different sexes, races, and marital statuses.

Table 2.2: Heterogenous Tax Salience Effects on Alcohol Demand

K	Income		Education		Sex	Race	Marital Status		Age	
	(1)		(2)	(3)			(5)	(6)	(7)	(8)
	Low-income	High school grad	College grad	Female	White	Married	Young	Old		
Excise tax	-1.485** (0.566)	-1.135* (0.600)	-1.534*** (0.572)	-1.569*** (0.557)	-1.240 (0.747)	-1.013 (0.653)	-1.355** (0.575)	-1.429** (0.621)		
Sales tax	-0.704 (2.373)	0.557 (2.570)	-0.449 (2.408)	-0.948 (2.515)	-0.547 (2.632)	-1.647 (2.789)	-0.833 (2.380)	-1.011 (2.426)		
Excise $\times K$	0.528 (0.391)	-0.303 (0.222)	0.478* (0.267)	0.324** (0.155)	-0.556 (0.652)	-0.575* (0.328)	-0.390 (0.331)	0.195 (0.384)		
Sales $\times K$	-0.685 (0.883)	-1.617* (0.924)	-1.228* (0.614)	0.304 (0.610)	-0.075 (1.830)	1.221 (1.364)	0.332 (0.516)	1.310*** (0.459)		
H_0 : Excise = Sales p -value	0.765	0.555	0.688	0.823	0.805	0.839	0.844	0.879		
H_0 : Excise $\times K$ = Sales $\times K$ p -value	0.192	0.168	0.011	0.974	0.734	0.155	0.368	0.134		
Observations	1,455,129	1,455,129	1,455,129	1,455,129	1,455,129	1,455,129	1,455,129	1,455,129		

Notes: 1984 - 2003 Behavioral Risk Factor Surveillance System data. All prices and taxes adjusted to 2000 dollars. Standard errors are clustered at the state level in parentheses. All specifications are weighted and include individual demographic characteristics (income, an indicator for female, an indicator for being pregnant, an indicator for being white, an indicator for having graduated high school, an indicator for having graduated college, an indicator for being married, an indicator for being unemployed, and age) and state, year, and calendar month fixed effects. State-specific linear time trends, income trends, and quarterly state income and log monthly unemployment trends are also included. Second-, third-, and fourth-order age polynomials are included in the regression but not displayed. Outcome variable: log total number of drinks consumed by an individual in the previous month (including 0 if the individual reported not drinking any) plus 1. Excise tax refers to $\ln(1 + \text{ad valorem excise tax rate})$. Sales tax refers to $\ln(1 + \text{sales tax rate})$. The p -values are for the test of equality in the coefficients specified by H_0 .

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Finally, I consider heterogeneity across age groups. For this, I define two new indicators. First, I use an indicator of if a consumer is 24 years old or younger. I choose this cutoff because by the time people are 25, the vast majority are finished with school and are either in the labor force or have a working spouse.²⁵ Several consumers under the age of 24 are still in college and could possibly have different drinking behavior as a result. It is possible that they could be less attentive to tax changes due to having less expenses and potentially being funded by their parents. This may lead them to be less financially aware than older consumers who most likely work for their money.²⁶ I also use an indicator of if a person is 65 or older. This is chosen to correspond with the typical retirement age in the United States. It seems possible that consumers could become more aware of their monthly balances and the prices that impact their expenditures when they no longer have a steady income.

Table 2.2 presents the results of this analysis. All specifications include both state economic conditions and income trends. Column (1) presents the results for heterogeneity between poor consumers and wealthier consumers. Columns (2) and (3) show the estimates for heterogeneity across levels of education. Columns (4), (5), and (6) show the estimates for heterogeneity across sex, race, and marital status, respectively. Columns (7) and (8) show the estimates for heterogeneity across age.

A few results stand out from this analysis. First, as predicted, education is positively related

²⁵ A 2002 report by the National Center for Education Statistics reports that well over half of all undergraduates are 25 years old or younger. Though a large percentage are actually older than this, it is hard to rule out that these students did not work before going to college, which might make them fundamentally different in their consumption decisions.

²⁶ There is a possible competing story that young consumers may be more careful with their money, since they have less, and thus be more sensitive to tax levels. Then, when they get their first real paycheck they become less sensitive to tax changes. In light of not finding any heterogeneous saliency responses due to income, I consider this alternate scenario unlikely.

with salience. Those with high school diplomas respond significantly more negatively (with a difference in elasticities of 1.617) to increases in the sales tax rate than those without. Additionally, consumers with college degrees respond even more negatively (with a difference in elasticities of 1.288) to increases in sales taxes. Furthermore, consumers with college degrees have a significantly different attention gap from consumers without college degrees. More educated individuals are more tax salient than less educated individuals, at least with respect to alcohol consumption. This offers support for the calculation cost hypothesis of tax salience.

Another notable result is that, while young consumers do not seem to differ much in tax responses from the general population, consumers older than 64 do. In fact, these older consumers respond significantly less to changes in the sales tax rate than their younger counterparts. While at first, this may be a little surprising, this result seems to be driven primarily by the fact that at least in the BRFSS, these older individuals are significantly less likely to have a college degree or a high school diploma. As such, they should be expected to have similar tax responses to individuals with lower levels of education.

Interestingly, I find little evidence of any heterogeneous responses in alcohol consumption among consumers of different income levels.²⁷ Though low-income consumers seem to have a more negative response to increases in sales taxes than high-income consumers, the difference is insignificant. Furthermore, the difference in attention-gap between low-income and high-income consumers is also insignificant. Some of this might be attributable to my larger standard errors, but this result stands in contrast to Goldin and Homonoff's findings with respect to cigarettes. One possible explanation might be related to the fact that poor consumers are more likely to less

²⁷This finding also holds if I run a more flexible model allowing for heterogeneous responses among consumers of every income quartile.

educated, which acts to reduce their tax salience. If having a lower income works to increase tax awareness, then these effects will partially cancel out.

2.6.3 Robustness Tests

2.6.3.1 Including Alcohol Prices

I now proceed to test the robustness of my main results. One major shortcoming of the data is the lack of state alcohol prices. While a change in the excise tax should necessarily result in a change in the sticker price of alcohol, alcohol sellers may still under- or over-adjust the price to a certain degree. This could bias my results, particularly the coefficient on the excise tax. While I do not have state-level prices, the Brewer's Almanac estimates the average sticker price of a six-pack of beer for the entire country every year over the range of my data. I combine this with monthly regional consumer price index (CPI) data specifically for alcoholic beverages to get an estimate of the regional price of a six-pack of beer for each year and region in my data set.²⁸ I then reestimate equation (2.1) including regional beer prices in a variety of ways.

Table 2.3 shows the results of including the regional price of beer in my main specification. Column (1) restates the primary results without prices. Column (2) adds the log regional price of beer to the covariates. This has small effects on the estimated demand elasticities with respect to excise taxes and sales taxes, but ultimately the estimated effect of price on beer is insignificant. The fact that the excise tax elasticity is now insignificant is due in part to the fact that excise taxes are highly correlated with the price of beer.

Because a state's excise tax directly influences the average regional price of beer, it might

²⁸This data comes from the Bureau of Labor Statistics. The 4 regions are Northeast, Midwest, South, and West. To adjust the average price to regional price, I multiply the average national price by the ratio of the regional CPI for alcohol to the national CPI for alcohol.

Table 2.3: Effect of Taxes on Alcohol Demand (Controlling for Regional Alcohol Prices)

	(1)	(2)	(3)	(4)
Excise tax	-1.404** (0.592)	-1.056 (0.757)		-1.386** (0.616)
Sales tax	-0.794 (2.401)	-0.945 (2.339)	-1.181 (2.482)	-0.762 (2.401)
Price		-0.518 (1.087)	-0.703 (1.030)	
Economic conditions	X	X	X	X
Income trends	X	X	X	X
H ₀ : Excise = Sales <i>p</i> -value	0.820	0.964	-	0.814
Observations	1,455,129	1,455,129	1,455,129	1,455,129

Notes: 1984 - 2003 Behavioral Risk Factor Surveillance System data. All prices and taxes adjusted to 2000 dollars. Standard errors are clustered at the state level in parentheses. All specifications are weighted and include individual demographic characteristics (income, an indicator for female, an indicator for being pregnant, an indicator for being white, an indicator for having graduated high school, an indicator for having graduated college, an indicator for being married, an indicator for being unemployed, and age) and state, year, and calendar month fixed effects. State-specific linear time trends, income trends, and quarterly state income and log monthly unemployment trends are also included. Second-, third-, and fourth-order age polynomials are included in the regression but not displayed. Outcome variable: log total number of drinks consumed by an individual in the previous month (including 0 if the individual reported not drinking any) plus 1. Excise tax refers to $\ln(1 + \text{ad valorem excise tax rate})$. Sales tax refers to $\ln(1 + \text{sales tax rate})$. Column (1) shows results from the main specification. Column (2) adds a control for the log average regional price of a 6-pack of beer. Column (3) uses log regional price in place of the excise tax. Column (4) uses a recalculated excise tax rate based off of regional prices. The *p*-values are for the test of equality in the coefficients specified by H₀.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

be more sensible to not include both the excise tax rate and the price of beer. Column (3) includes log regional prices in place of the excise tax. Interestingly, in this case, the coefficient on log price is not significant. This is not unexpected since the prices are regional and do not reflect changes in the prices the consumers actually face as well as excise taxes do. Finally, in Column (4), I recalculate the excise tax rate using my estimates of regional average prices in place of the national average prices. The results of using the new excise tax rates are highly similar to my main results. Overall, my model is fairly robust to including regional beer prices, suggesting that changes in excise taxes are in fact capturing changes in prices.

2.6.3.2 Other Robustness Checks

There are some other potential concerns with my main results. First, it is possible that my results are being driven in some part by some of the sample restrictions I made, in particular my decision to only focus on consumers over the age of 21. It is likely that over the period of this sample, consumers under the age of 21 also purchase and consume alcohol over this time period. Column (2) of Table 2.4 shows the results of expanding my sample to include all consumers over the age of 18. This does not seem to have much effect on the estimated tax elasticities. It is also possible that the decision to not explicitly exclude pregnant women from my sample could have some impact.²⁹ Removing them from the sample has no effect on my estimates, as seen in column (3).

State excise taxes on alcohol are often justified as trying to reduce alcohol consumption. There are several other policies states can implement to achieve this goal, potentially at the same

²⁹Pregnant women are much less likely to drink than their non-pregnant counterparts. I model for this in my main specification with a pregnancy dummy.

Table 2.4: Effect of Taxes on Alcohol Demand: Robustness Tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Excise tax	-1.404** (0.592)	-1.593** (0.665)	-1.446** (0.587)	-1.815*** (0.662)	-1.200* (0.662)	-1.454*** (0.506)	-1.453*** (0.503)
Sales tax	-0.794 (2.401)	-0.907 (2.422)	-0.810 (2.446)	-0.976 (2.290)	-0.743 (2.993)	0.790 (2.915)	-0.067 (3.049)
Economic conditions	X	X	X	X	X	X	X
Income trends	X	X	X	X	X	X	X
H_0 : Excise = Sales p -value	0.820	0.802	0.816	0.729	0.887	0.464	0.649
Observations	1,455,129	1,503,418	1,438,289	1,455,129	975,579	1,455,129	1,455,129

Notes: 1984 - 2003 Behavioral Risk Factor Surveillance System data. All prices and taxes adjusted to 2000 dollars. Standard errors are clustered at the state level in parentheses. All specifications are weighted and include individual demographic characteristics (income, an indicator for female, an indicator for being pregnant, an indicator for being white, an indicator for having graduated high school, an indicator for having graduated college, an indicator for being married, an indicator for being unemployed, and age) and state, year, and calendar month fixed effects. State-specific linear time trends, income trends, and quarterly state income and log monthly unemployment trends are also included. Second-, third-, and fourth-order age polynomials are included in the regression but not displayed. Outcome variable: log total number of drinks consumed by an individual in the previous month (including 0 if the individual reported not drinking any) plus 1. Excise tax refers to $\ln(1 + \text{ad valorem excise tax rate})$. Sales tax refers to $\ln(1 + \text{sales tax rate})$. Column (1) shows results from the main specification. Column (2) adds in consumers who are younger than 21. Column (3) removes all pregnant women from the sample. Column (4) adds in controls for state policies designed to reduce alcohol consumption. Column (5) restricts the sample to only states that exempt food from the sales tax. Columns (6) and (7) replace the current sale tax rate with a rate lagged by one and three years, respectively. The p -values are for the test of equality in the coefficients specified by H_0 .

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

time as increases in the excise tax. To show that my results are being driven by changes in the excise tax and not similar policies, I revise my specification to include a series of dummy variables controlling for various state policies that have been implemented across several states during this time period.³⁰ The results of this analysis, seen in column (4) show that including these state alcohol law controls has no effect on estimates.

Another possible concern is that states with a different sales tax rate for alcohol than their standard sales tax rate might have different levels of consumer response to tax increases. Though

³⁰These include having a lower legal drinking age than 21, having a lower blood alcohol content (BAC) limit for youth drivers, having a lower BAC level for any drivers, and administrative license revocation laws.

I cannot observe which states historically had different sales tax rates for alcohol, I can restrict the sample to include only those states that exempted food from the sales tax in 2002. Doing so will allow me to examine states where the relevant alcohol sales tax is surely different from the sales tax on similar goods. These states also have at least two distinct sales tax rates: the sales tax for alcohol and a sales tax of zero for food. If the structure of the sales tax has a significant effect on consumer salience, then running a regression on this restricted sample should show drastically different responses to changes in the tax rates. The results are presented in Column (5) of Table 2.4 and show that the coefficients on the sales and excise tax rates are not appreciably different from my main results. This suggests that there is not much difference in the salience effects in states where alcohol has a different sales tax rate from other food.

Finally, it is possible that consumers are unaware of changes in the sales tax rate immediately, but learn about them over time as they make purchases. To test for this possibility, I rerun my main specification using a lagged sales tax rate in place of the actual sales tax rate. In column (6), I lag the sales tax rate by one year, and in column (7), I lag the sales tax rate by three years. In both cases, my main results are robust to lagging the sales tax rate. This supports the tax salience literature and suggests that some other mechanism is accounting for the under-response to sales taxes.

2.7 Discussion

The analysis presented in this paper provides some evidence of tax salience responses in the market for alcohol consumption. I estimate that for a one percent increase in the excise tax rate, consumers decrease alcohol consumption by 1.404 percent. The sales tax elasticity of demand for alcohol is roughly half this size, though it is not statistically different from zero. My estimates of

the excise tax elasticity are in line, though a little larger than previous estimates provided by CLK (2009). My sales tax elasticity estimates are much larger.³¹

Additionally, while I do not find heterogeneous responses across consumers of different income levels, I do show evidence of other heterogeneous responses. In particular, more educated consumers are more responsive to sales taxes than less educated consumers, and as a result have a smaller attention gap when it comes to taxes. While I also find that older consumers are less responsive to sales taxes, this seems to be driven by the fact that older consumers are also less educated. Though my results hold up to a variety of robustness checks, there are some factors that could be leading to biased results.

It may be the case that my assumption that alcohol consumption trends follow beer consumption trends closely could be leading to some estimation bias. This would be especially true if excise taxes on wine and liquor differ drastically from excise taxes on beer.³² Unfortunately, differentiating across types of alcohol consumption in the BRFSS is not possible after 1988 making any analysis seeking to differentiate across beer, wine, and liquor difficult. Additionally, while the prices I use to calculate excise tax rates are representative of the average price of a six pack bought from retailers, a large quantity of alcohol is purchased in restaurants and bars and is subject to higher prices. Being able to distinguish across the type of establishment a consumer purchases alcohol from would provide a cleaner analysis, though the BRFSS does not collect this data.

Though the exact mechanism that salience occurs through is still unclear, my results add to a growing literature supporting the cognitive cost hypothesis. CLK (2009) suggest that there

³¹My estimated elasticities are smaller in magnitude than those by Goldin and Homonoff (2013), though this is likely due, at least in part, to differences between consumers of alcohol and cigarettes.

³²However, as CLK (2009) show, changes in beer excise tax rates are highly correlated (on the order of 86%) with changes in other alcohol tax rates.

may be a cognitive cost to consumers that results from trying to calculate the after-tax cost of a purchase. If this cost results in a high enough level of disutility, then the consumer would rather “pay” for a miscalculation than spend the time to get an accurate measure of how much they will owe after-tax. Several papers outside the economic literature have used controlled experiments to attempt to provide evidence for this hypothesis.³³ My results support this hypothesis by showing that consumers with higher levels of education (and by assumption, lower cognitive costs) are more responsive to increases in the alcohol sales tax than those with lower education. My results also discount the potential mechanism that it takes time for consumers to learn about changes in sales taxes.

Finally, a possibility that I have not accounted for is that my results could be consistent with other hypotheses about consumer behavior instead of tax salience theory. First, it is possible that consumers are completely unaware of the actual tax rates. They may not even be aware of what goods are taxed. This is not a tax salience model, but an imperfect information model.³⁴ Another possibility is that since sales taxes increase the prices of all goods and excise taxes increase the price of alcohol relative to other goods, “typical” supply and demand responses to relative price changes could be explaining the results. My data is not able to effectively control for this possibility of income effects driving the results.³⁵

As such, my results can be taken to be suggestive of both tax salience effects in the general

³³For example, Hayashi, Nakamura, and Gamage (2013) tests the cognitive limitations hypothesis against other hypotheses such as price-anchoring and framing preferences using a controlled experiment. Their results support the cognitive costs theory.

³⁴While I cannot control for this using my data, CLK (2009) find in survey evidence that consumers are in fact very aware of what the relevant sales tax rate is. This suggests that the imperfect information model is not representative of the effects that I find.

³⁵CLK (2009) provide some evidence against this possibility, though they are not able to completely disprove it.

population and heterogeneous tax salience effects across education levels. As for the policy implications of these two types of taxes, it seems that sales tax rates on alcohol could possibly be a more efficient way to raise revenue than excise taxes since consumers seem to not respond significantly to sales tax changes. However, there is mixed evidence on whether alcohol taxation is more or less regressive in the presence of salience effects. On the one hand, high-income consumers seemingly purchase more alcohol than low-income consumers, suggesting that the tax may be less regressive than generally thought. On the other hand, consumers with education levels generally associated with higher wages appear to have higher levels of tax salience, implying the tax could be more regressive. Extrapolating my results to general consumption should be treated cautiously, of course, as there are many differences in the alcohol market compared to other consumption markets.

Chapter 3

(Undocumented) Kids in America: The Direct and Indirect Impacts of Immigration Reform on the Children of Undocumented Immigrants

3.1 Introduction

At midnight on January 20, 2018, the United States government shutdown for the first time since 2013. Though the shutdown lasted less than 72 hours, it brought a long-brewing debate on undocumented immigration to its most recent head. In fact, the key issue that led to the failure to pass legislation to fund government operations was an impasse over an extension of the Deferred Action for Childhood Arrivals immigration policy, better known as DACA. Of course, DACA is far from the only immigration policy to be featured in the headlines over the past decade. Since 2006, several states have implemented policies, such as E-Verify mandates, “Show Me Your Papers” (SMYP) style law enforcement provisions, and entering into 287(g) agreements with Immigration Customs and Enforcement (ICE).

These state policies are designed to identify and remove unauthorized immigrants from the United States, though they do so in different ways. SMYP and 287(g) agreements primarily operate through law enforcement by empowering police to determine an individual’s legal status (in the case of SMYP) and initiate the deportation process (in the case of 287(g) agreements).¹ E-Verify

¹Without such policies, police are typically not allowed to ask about immigration status and must wait for an ICE

mandates, instead, work through incentivizing employers against hiring undocumented workers. E-Verify provides a more stringent legal status background investigation and places penalties on employers who do not comply with using the system. These types of reforms have been found to significantly reduce the population and employment of undocumented workers in the states that have implemented them.

Unlike these types of policies, which have a negative economic impact on undocumented immigrants, DACA provided a legal path for young undocumented immigrants to live and work in the United States, provided certain eligibility conditions were met.² One of the key conditions for DACA eligibility is completing high school. As such, the policy theoretically incentivizes eligible children to finish secondary school. Beyond high school, by granting permission to legally stay in the United States, DACA reduces barriers for eligible immigrants to find and keep jobs and to obtain higher education if they choose.

There is an extensive and growing literature on the impacts of various state immigration reforms on undocumented immigrants. Recently, papers such as Amuedo-Dorantes and Bansak (2012), Orrenius and Zavodny (2015), and Borjas (2017) all find evidence that E-Verify mandates significantly reduce the economic well-being of working-age undocumented immigrants, particularly undocumented men.³ Other studies, such as Orrenius and Zavodny (2016), show that undocumented immigrants are leaving states with E-Verify mandates. Furthermore, Amuedo-Dorantes and Pozo (2014) and Hoekstra and Orozco-Aleman (2017) both provide evidence that SMYP reduced the number of border crossings from Mexico into the United States. Finally, in Chapter 1, I

agent to take custody of an undocumented immigrant to start the deportation process.

²The exact conditions for DACA eligibility will be discussed in Section 2.

³The definition of working age varies across these studies, but is completely captured by an age range of 16–64.

show that SMYP provisions, in addition to E-Verify mandates, reduce the employment and wages of undocumented men and increase the number of ICE detainers issued on suspected undocumented immigrants. Chapter 1 also shows that state GDP per capita falls in states that implement E-Verify mandates or SMYP reforms.

All these studies focus on the effects of immigration reform on undocumented adults. This paper takes the logical extension of considering the implications of immigration reforms on their children. In particular, I look at human capital accumulation and education-related outcomes and decisions for the children of undocumented immigrants. In 2012, roughly 11.2 million unauthorized immigrants lived in the United States.⁴ While the vast majority of these immigrants are adults, nearly one million are children or teenagers. However, this severely understates the number of children living with undocumented parents. Capps, Fix, and Zong (2016) estimate that between 2009 and 2013, 5.1 million children in the US lived with undocumented parents. Of this group, about 4.1 million were US citizens.⁵ This represents a sizable population group worthy of more detailed economic study. Even though policies such as E-Verify mandates and SMYP typically target adults, the large negative impact these policies have on undocumented immigrants might also be felt by their children.⁶ For instance, if undocumented parents are having more difficulty finding steady employment and supporting the family, their children might be incentivized to leave school and begin working to supplement the family income. This effect might be stronger for children

⁴Source: Passel et al (Pew Research Center 2014). A report by the DHS provides a similar estimate.

⁵By law, anyone born in the United States is a US citizen. This applies to children with undocumented parents as well.

⁶Even SMYP has a more limited impact on children than adults. Children are not required to carry identification or immigration papers and thus, cannot be detained by police for violating this provision. One notable exception is Alabama HB 56, which in addition to instituting SMYP also requires school districts to submit tallies of the number of undocumented immigrants attending school to state officials. The details and possible implications of this policy will be discussed in more detail in Section 2.

who are US citizens, given that they do not face the same constraints as their parents in states with E-Verify mandates or SMYP laws.

Though this paper is the first to consider the impact of these types of immigration reforms on the children of undocumented immigrants, several other papers have looked at the effects of other policies on similar, though not identical, groups. Lately, the majority of the literature has focused on the impact of DACA on young immigrant adults.⁷ Amuedo-Dorantes and Antman (2017) find that DACA reduces the probability of enrollment in college and increases the likelihood of employment among eligible individuals. Pope (2016) also finds that DACA increases employment among eligible individuals, though he finds little evidence of any effect on higher education attainment. Hsin and Ortega (2017) find similar results. These studies, and others like them, are constrained to only considering the effects of DACA on individuals older than 18. An important criteria for DACA eligibility is the completion of high school or current enrollment in school. Given this requirement, it is certainly policy-relevant to understand how much DACA incentivizes potentially eligible undocumented children to invest in their education and ultimately complete high school. Kuka, Shenhav, and Shih (2018) address this question by considering the impact of DACA on high-school-aged immigrants. They find that DACA significantly increases high school graduation rates as well as college attendance and employment among noncitizen immigrants that meet DACA's other requirements. They also find that DACA reduces teenage pregnancy among this group.

One of the shortcomings of the existing DACA literature is its imprecise measure of un-

⁷DACA is not the only policy that has been studied in this context. For instance, Bozick, Miller, and Kaneshiro (2016) find that Mexican-born noncitizen children are less likely to attend college in states that deny in-state tuition to undocumented immigrants.

documented, and thus potentially eligible, youth. These studies primarily consider the effects of DACA on all immigrants who are not US citizens. Generally, they use a difference-in-difference approach that compares non-citizens who meet eligibility requirements to non-citizens who do not, though Kuka, Shenhav, and Shih (2018) compare noncitizen youth to naturalized citizen youth. The main issue with this approach is that it drastically overstates the number of potentially DACA-eligible individuals by including groups of people who are very unlikely to be undocumented. My biggest contribution with this study is an arguably better approach to proxy for the immigration status of children by their using parents' immigration status.⁸ Looking explicitly at children with undocumented parents also provides a better control group of DACA-ineligible individuals, in the sense that they will have more similar family backgrounds.⁹ This approach to identifying undocumented children also allows me to take my analysis of DACA one step further by focusing on DACA's impact within families where some children are DACA-eligible, apart from the education requirements, while their siblings are not. This will arguably provide cleaner estimates of the true impact of DACA on education decisions by removing the threat of endogeneity related to any unobservable traits about an individual's family.

My results show that DACA has numerous positive impacts on its potential recipients. For one, it increases their enrollment rate in school and ultimately, their completion of high school. I also find some evidence that DACA increases the college enrollment rate and employment rates of eligible youth. However, policies such as Alabama HB 56, a particularly strict version of SMYP, have large negative consequences for the children of undocumented immigrants. I find that this

⁸Adult immigration status is still a proxy in surveys such as the ACS, but it has more precedent in the literature. I will go into greater detail about the best proxy for an undocumented adult in Section 3.

⁹In fact, I have two control groups I can use: US citizens with undocumented parents and non-citizens who miss out on one of DACA's eligibility requirements with undocumented parents.

policy greatly increases the high school dropout rate and teen pregnancy rate among these children. While it also reduces the employment rate of noncitizen children, citizens with undocumented parents actually increase their employment. This seems to be, in part, to offset some of the negative economic consequences that SMYP and E-Verify have on their parents.

The remainder of this paper is organized as follows. Section 2 discusses the relevant background and policy details of DACA and state immigration reforms. Section 3 provides an overview of the data used for this analysis. Section 4 presents the methodology and results for my analysis of the effects of DACA on the children of undocumented immigrants. Section 5, similarly, discusses the methodology and results for my analysis of the effects of state-immigration reforms, in particular SMYP-style reforms, on the children of undocumented immigrants. Section 6 concludes by discussing the implications of my findings.

3.2 Background on Immigration Policy

3.2.1 Deferred Action for Childhood Arrivals

DACA has its roots in the Development, Relief, and Education for Alien Minors (DREAM) Act. The DREAM Act itself was originally proposed in 2001 with the intent of providing a permanent pathway to legalization for undocumented immigrants who arrived in the US when they were children, provided they met a specific set of criteria. However, despite a history of various rewrites, the bill continually failed to pass through the legislative branch. Because of this failure to gain traction for the DREAM Act, on June 15, 2012, President Barack Obama enacted DACA via an executive order.

DACA ultimately has similar, though less permanent, policy goals to the DREAM Act. Notably, recipients of DACA are able to legally stay in the US for a period of two years. DACA

also provides recipients with a permit to legally work in the US. Though these benefits only last for a two year period initially, DACA recipients are eligible to apply for a renewal that will last another two years. This process could be repeated as many times as necessary provided that the individual still meets all the prerequisite conditions for DACA eligibility.

According to US Citizenship and Immigration Services (USCIS), the criteria for DACA eligibility are: (1) an eligible individual must have been under the age of 31 on June 15, 2012; (2) he or she must have arrived in the US before age 16; (3) he or she must have lived continuously in the US since June 15, 2007; (4) he or she must have been physically present in the US at the time of application and on June 15, 2012; (5) he or she did not have legal immigration status on June 15, 2012; (6) he or she has graduated from high school (or holds an equivalent degree) or is currently enrolled in school; and (7) he or she has no criminal record and poses no threat to national security. Applicants must also be at least 15 years old and pay a fee of \$495. While meeting these criteria does not guarantee that an application is approved, in practice, most eligible individuals who applied were ultimately granted DACA.¹⁰

While DACA was enacted on June 15, 2012, applications were not accepted until August 15, 2012. The first applications were approved in September, 2012. Applications for renewal were first accepted on June 5, 2014. It is also worth noting that the ethnic distribution of DACA applicants closely mirrors the ethnic distribution of undocumented immigrants in general. Specifically, over 80% of DACA applicants have Hispanic origins.¹¹ Meanwhile, roughly 75% of undocumented

¹⁰Zong, Batalova, and Hallock (2018) report that as of September 30, 2017, 88% of the 906,693 initial applications for DACA that were accepted for consideration have been approved. The approval rate for applications for renewal are even higher at 92%.

¹¹Zong, Batalova, and Hallock (2018).

immigrants have Hispanic origins.¹²

3.2.2 State Immigration Reform

“Show Me Your Papers” and Alabama HB 56

Starting with Arizona, in 2010, several states passed comprehensive anti-undocumented immigration policies.¹³ These bills, though they differed somewhat in exact wording and scope, all contained numerous provisions designed to remove undocumented immigrants from the state. In each case, these provisions were immediately blocked by state courts for a review of their constitutionality.¹⁴ While most of the provisions included in these laws remain blocked to this day, four states (Alabama, Arizona, Georgia, and South Carolina) upheld the “Show Me Your Papers” (SMYP) provision.¹⁵ The SMYP provision requires police officers to make efforts to determine a person’s immigration status if there is reasonable suspicion that said person is unauthorized to be in the United States during any lawful stop or arrest. Any person found without proper identification is subject to arrest and detainment while his or her immigration status is determined.¹⁶

By the nature of US immigration law, the SMYP provision, for the most part, only directly impacts adults.¹⁷ However, Alabama included a provision in its omnibus immigration reform, Alabama HB 56, that school officials at all public high, middle, and elementary schools submit annual

¹²Passel et al (2014).

¹³These states include Alabama, Arizona, Georgia, Indiana, South Carolina, and Utah.

¹⁴These court cases took years to be resolved and even reached the US Supreme Court. For instance, Arizona passed its immigration reform bill, Arizona SB 1070, on April 23, 2010. The Supreme Court made its final ruling on SB 1070 on June 25, 2012.

¹⁵It is worth noting that several large police departments in Georgia, including the Atlanta Police Department publicly stated that they would not enforce SMYP.

¹⁶Proper identification does not have to be actual immigration papers. It can be as simple as a valid driver’s license.

¹⁷Under federal law, all non-citizens over the age of 14 in the United States for more than 30 days are required to register with the federal government.

tallies to state education officials of the suspected number of undocumented immigrants attending. Though the law does not prohibit undocumented students from attending school, anecdotal evidence points to several Hispanic families withdrawing their children from school immediately following the enforcement of HB 56 on September 28, 2011.¹⁸ These families reportedly cited a fear of drawing the attention of authorities as the motivating factor behind the withdrawals.

E-Verify Mandates

E-Verify is an internet-based program, run by the Department of Homeland Security and the Social Security Administration, that allows employers to verify the employment eligibility of new hires. The process has been quick and free for all employers in the United States to use ever since its inception in 1997. In part due to low-levels of voluntary take up, several states started mandating that all public employers and contractors working with public employers use E-Verify.¹⁹ A handful of states have taken this a step further and mandated that all employers, both public and private, use E-Verify.²⁰ The penalties for noncompliance in these states can range from fines to suspended business licenses.²¹

3.3 Data

The primary data for my analysis of the impact of various immigration policies on the children of undocumented immigrants comes from American Community Survey (ACS) IPUMS 1%

¹⁸This is despite assurances by superintendents that neither students nor their parents will be arrested for attending school and that the state is only trying to compile statistics. Source: The New York Times. October 1, 2011.

¹⁹Colorado was the first state to mandate public sector E-Verify usage in 2006. 20 other states have followed since then.

²⁰These states are Alabama, Arizona, Georgia, Mississippi, North Carolina, South Carolina, Tennessee, and Utah.

²¹For more detailed descriptions of the E-Verify process and SMYP laws, please see Chapter 1.

samples for 2005–2016.²² The ACS is a monthly cross-sectional survey administered by the U.S. Census Bureau, containing detailed responses on race, ethnicity, education, income, employment, and a wide array of other demographic variables. Notably the ACS contains data on an individual’s year of birth, year of immigration, country of origin, and citizenship status. These variables play an important role in identifying the subset of the survey population most likely to be undocumented and DACA-eligible.

The ACS does not contain direct information on an individual’s true legal status of immigration. I can, however, create a proxy for undocumented immigrants using a very common and widely-accepted method in the immigration literature. Following Passel and Cohn (2014), I consider undocumented immigrants to be represented in the ACS as all low-skilled, working-age, non-citizen Hispanic immigrants, excluding Cubans and Puerto Ricans.²³ “Low-skilled” refers to individuals with a high school education or less. “Working age” refers to all individuals between ages 18 and 64, in this context.

This proxy is useful for identifying undocumented adults, but less so for children. In particular, children under the age of 18 will have less than a high school education and by definition be considered low-skilled. Thus, all non-citizen, Hispanic immigrant children would be counted as undocumented, even if their parents are more highly skilled and, thus, less likely to be undocumented. While the prior literature focusing on DACA has been content with this definition, a better and more representative proxy for undocumented children can be constructed by taking advantage of the ACS’s ability to match family members living in the same household. The ACS contains

²²Though the ACS began collecting data in 2000, it is not considered a nationally representative survey until 2005.

²³Cuban immigrants typically have refugee status in the US. Puerto Ricans are US citizens.

data on each member of a household's relationship to the surveyed head of household. Using this, I can easily match children to their parents, and in doing so, can focus my analysis specifically on children with parents that meet the proxy for being an undocumented adult rather than all children without US citizenship.²⁴ This provides some extra advantages as well. First, it allows me to also analyze the effects of immigration policies on children who have citizenship but also have undocumented parents. Second, it allows me to compare the effects of immigration policy on siblings in the same household with different citizenship status or DACA-eligibility.²⁵

To measure an individual's DACA-eligibility, I focus on eligibility criteria (1)–(5). Using an individual's date of birth, I can calculate their age as of 2012, making sure they would not be older than 31 and violate criteria (1). Using age and date of immigration, I can calculate when an individual arrived in the US and how old they were at arrival, satisfying conditions (2)–(4). Finally, to satisfy criteria (5), I only allow the noncitizen children of likely-undocumented immigrants to potentially be eligible for DACA. I am not concerned with individuals having completed high school, since this is an outcome I consider. Thus, for my DACA analysis, I focus on effects of DACA on children who could become DACA-eligible if they satisfy criteria (6). Since the ACS contains no information on an individual's criminal history, I cannot control for criteria (7). Thus, even though my measure of the potentially DACA-eligible population is more accurate than the previous literature, it still encompasses individuals who would not be eligible to receive DACA either because of the criminal history component or because their parents are low-skilled but have

²⁴I primarily focus only on children with only undocumented parents. This can refer to households where both parents are undocumented or to single-parent households where the single-parent is undocumented.

²⁵The big drawback of this method of identifying undocumented children and young adults is that I am restricted to only looking at those who still live in the same household as their parents. There may be some selection as to the type of young adult who chooses to still live at home. I also miss out on analyzing undocumented children who might have already moved out or are living with other family members than their parents.

work visas or some other form of legal status.

For some of my analysis, I control for state-level economic indicators including the unemployment rate, government spending per capita, and new housing permits. These factors help control for any effect the business cycle might have on an individual's decision to stay in school or dropout to seek employment. These data come from the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS) survey, the Census Annual Survey of State Government Finances (SGF), and the Census Building Permit Survey.

3.4 Part I: The Effects of DACA

In theory, DACA should incentivize more potentially eligible students to complete high school. By granting temporary legal working status, DACA arguably increases the returns to a high school diploma for eligible youth.²⁶ Not only are more and better paying jobs likely available to DACA recipients, the reduction in stress from removing the fear of deportation should also be utility-increasing. Thus, DACA is predicted to increase enrollment and reduce the dropout rate among potentially eligible individuals age 18 and younger and increase the number of potentially eligible individuals older than 18 who have graduated high school.²⁷ DACA might also have an effect on student performance in school. Potentially eligible students might be incentivized to put forth more effort in school since the return to schooling increases if they achieve DACA-eligibility. On the other hand, DACA might encourage marginal students who would otherwise dropout to stay

²⁶Furthermore, recipients might have viewed DACA as more permanent than temporary since they could apply for renewals multiple times.

²⁷By potentially eligible, I mean the children of undocumented immigrants who meet all observable DACA criteria apart from the educational component. This also includes those younger than the age of 15 who could be eligible after their 15th birthday.

in school. This might reduce student performance. While I do not have information on test scores, I do have information on which grade a student is enrolled in. I use this to determine if a student is in the correct grade for their age, in other words, whether a student has been held back or not.²⁸

DACA, by increasing the returns to schooling (and the opportunity cost of childbirth for teenagers), might also be expected to reduce teenage pregnancy, an outcome I can measure for girls 15 or older.²⁹ Finally, DACA is very likely to have some effect on employment and college decisions for eligible youth. The higher return to a high school diploma should increase the job opportunities for DACA recipients, leading to an increase in employment. However, DACA might also provide increased opportunities and incentives to attend college. These effects might work against one another if DACA recipients choose to either work or attend college. However, both effects might be positive if DACA recipients work while attending college.³⁰

3.4.1 Methodology

3.4.1.1 Comparing Across Families

To empirically test these predictions using the ACS, I utilize two different approaches. In the first approach, I use a difference-in-difference specification, comparing outcomes for the children of undocumented immigrants who are potentially eligible for DACA to a control group who are not. There are two possible control groups I have to choose from. The first is children who have undocumented parents and do not have US citizenship but are not eligible for DACA because

²⁸This data is available from 2008 onwards. For this measure, I allow for the possibility of students not starting kindergarten until they are 6. Hence a 6 year old (or younger) student in kindergarten would be in the correct grade, but a 7 year old would not. This pattern continues for all grades through 12th, where an 18 year old (or younger) would be in the correct grade.

²⁹The exact question in the ACS asks if an individual has given birth in the past year.

³⁰One reason for this is that DACA recipients might have to work to help finance their education.

of one of the other criteria.³¹ The other possible control group is children with US citizenship and undocumented parents. Because the pre-DACA trends are much more similar for potentially DACA-eligible children and US citizen children than for noncitizen ineligible children, I choose citizens as the control group for my analysis.³² To give an example of this, Figure 3.1 shows the average enrollment rates by year for these groups.³³ Clearly, prior to 2012, US citizens have a more similar trend.³⁴

With the control group established, I estimate the empirical effects of DACA using the following equation:

$$Y_{iast} = \alpha + \beta Eligible_i \times After_t + \gamma Eligible_i + \delta X_{iast} + \zeta Z_{st} + \mu_a + \mu_s + \mu_t + \mu_{st} + \epsilon_{iast} \quad (3.1)$$

Y_{iast} represents the outcome, say enrollment in school, for child i who is a years old, living in state s and year t . X represents a vector of individual covariates, such as sex, English proficiency, having a recorded disability, household size, household income, having both parents present, and number of siblings.³⁵ Z represents a vector state-level variables including the state unemployment rate to capture any effects the business cycle might have on the outcomes of interest. Z also includes a set of state immigration policy indicators to control for the possible effects of E-Verify

³¹Typically these children were either too old when they arrived in the United States or have not been in the United States long enough to be eligible.

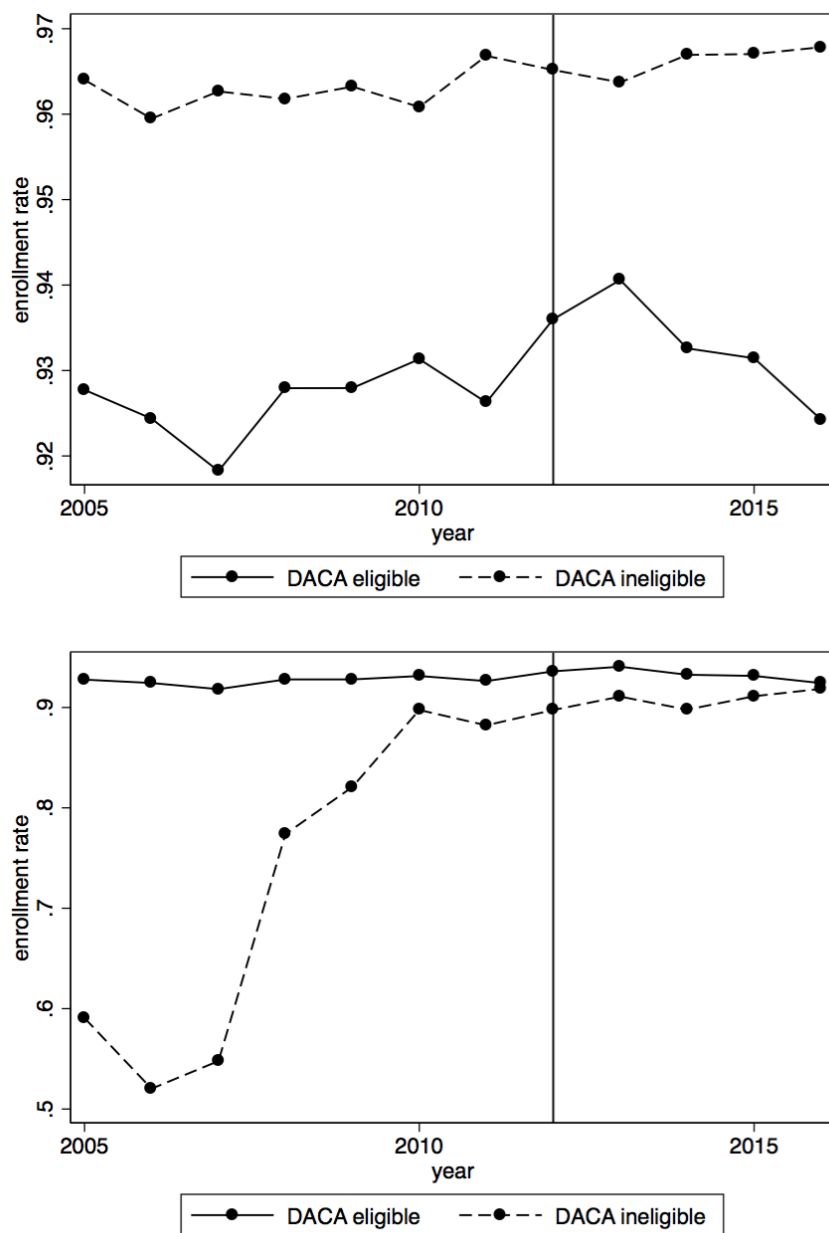
³²This has the added benefit of giving me larger sample sizes, which will be even more useful when comparing siblings within a household.

³³Graphs for other outcome variables show a very similar story. These graphs are not shown here to conserve space but are available upon request

³⁴This confirms the reasoning given by Kuka, Shenhav, and Shih (2018) in regards to their decision to use teenage naturalized citizens as their comparison group rather than DACA-ineligible non-citizens.

³⁵The disadvantage of using US citizens as a control group is that I cannot control for the number of years that the immigrant children have lived in the US as a measure of assimilation. This probably has some effect on educational decisions, whether from the child or parents' perspectives. I use English speaking ability as a proxy to measure assimilation.

Figure 3.1: School Enrollment Rates Among Children with Undocumented Parents



The top figure compares the average school enrollment rate for potentially DACA-eligible children ages 5–18 of undocumented immigrants to US citizen children ages 5–18 of undocumented immigrants. The bottom figure compares the average school enrollment rate for potentially DACA-eligible children ages 5–18 of undocumented immigrants to ineligible noncitizen children ages 5–18 of undocumented immigrants.

mandates and SMYP laws. The coefficient of interest is on the interaction of $Eligible_i$, which equals one if child i is potentially eligible for DACA (or actually eligible if they are older than 15 and meet the education requirements), and $After_t$ which equals one if the year is after 2012. The equation also includes age, state, and year fixed effects as well as state-specific linear time trends to capture any possible differences in the education patterns of undocumented immigrants living in different states. In order to make my estimates more representative of the entire population of undocumented children, I weight all my regressions using the ACS individual person weights. Because the ACS does not contain information on which month an individual was surveyed in, I omit all observations from the year 2012 from my analysis since it is unclear whether DACA was in effect or not for these individuals.³⁶

For my identification strategy to be valid, I need for the control group to exhibit parallel trends during the pre-period and for DACA to be reasonably exogenous from other factors that might explain changes in these children's education behaviors. As I discussed earlier, the pre-DACA trends for citizen children with undocumented parents are reasonably similar to the treated group's. Regarding second point, DACA was enacted swiftly. Given the long history of the failure of the DREAM Act, it seems unlikely that parents with children especially suited to take advantage of DACA would have moved to the US. DACA seems to have been largely unanticipated, especially given that participants would have had to anticipate DACA by five years to arrive before June 15, 2007. DACA-eligibility also is more or less exogenous from the child's choices, apart from education. Undocumented children almost always move into the US with their parents and so could not have chosen the timing of their entry to specifically meet all of DACA's eligibility requirements.

³⁶Including 2012 as a treated year or a control year does not have much impact on my point estimates.

3.4.1.2 Comparing Within Families

Despite numerous factors suggesting that potential DACA-eligibility is exogenous, there could still be concern that parents might specifically choose to remain in the US if they know or suspect that their children will gain more from DACA than others. If more able children and their families select to remain in the US to take advantage of DACA, then any estimates using my prior strategy are likely to overstate the true impact of DACA. To address this concern, I turn to a difference-in-difference strategy designed to compare outcomes for siblings within the same family. Specifically, I compare the children of undocumented immigrants who are potentially DACA-eligible to their citizen siblings who are not. Again, I choose to focus on their citizen siblings rather than noncitizen siblings because the pre-trends are noticeably more similar; Figure 3.2 provides an example of this. This sibling-comparison strategy has the added benefit of also removing concerns about unobservables such as a family's overall valuation of education that might influence a child's outcomes.

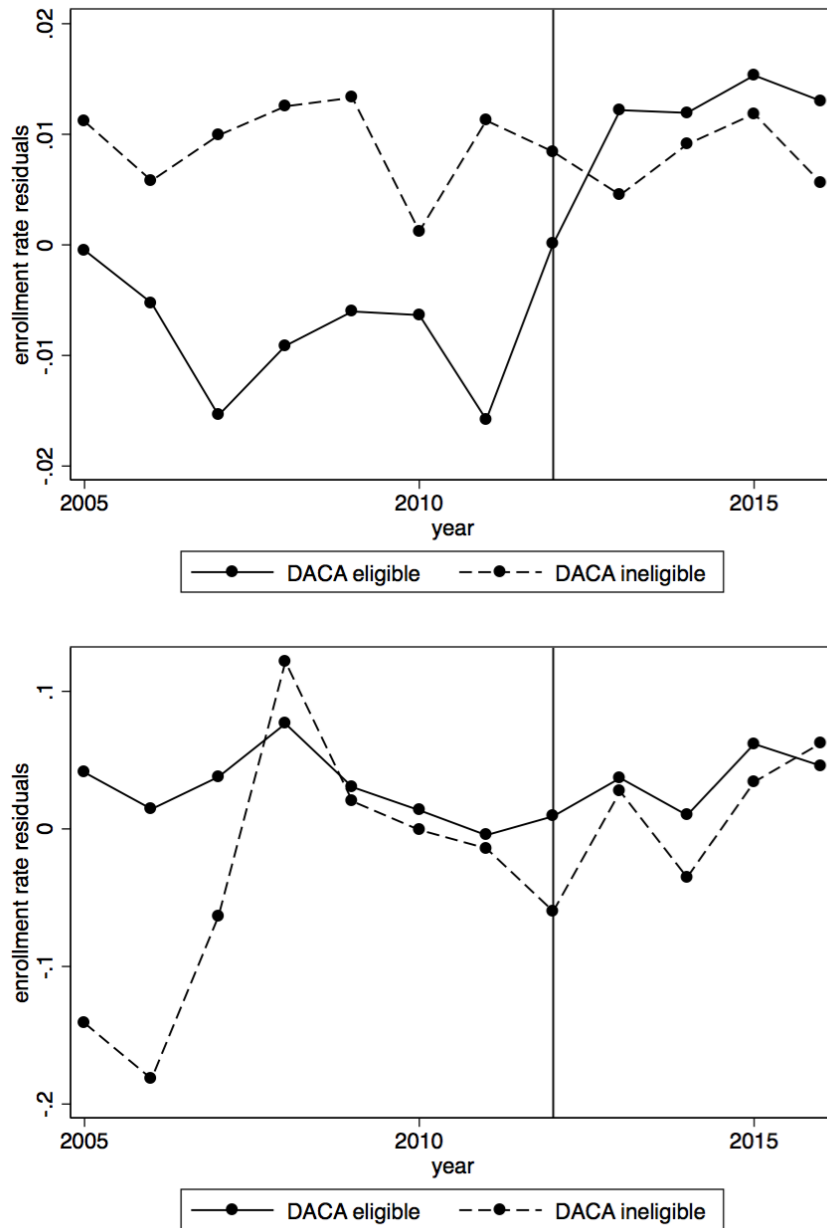
To compare outcomes within families, I use a modified version of equation (3.1):

$$Y_{iab h} = \alpha + \beta Eligible_i \times After_t + \gamma Eligible_i + \delta X_{iab h} + \mu_a + \mu_b + \mu_h + \epsilon_{iab h} \quad (3.2)$$

Now, $Y_{iab h}$ represents the outcome for child i who is a years old, whose birth order position is b and who lives in household h . Because the ACS is a repeated cross-sectional dataset rather than a panel dataset, the state s and year t that an individual lives in are contained in their household identifier. X is a vector of observable characteristics that can vary across siblings living in the same household.³⁷ The coefficient of interest remains the same as in equation (3.1), and $Eligible_i$

³⁷These include gender, disability status, and English-speaking ability.

Figure 3.2: School Enrollment Rates Among Siblings with Undocumented Parents



The top figure compares the average residuals of regressing school enrollment rate on sex, disability status, English-speaking ability, and age, birth order, and state fixed effects for potentially DACA-eligible children ages 5–18 of undocumented immigrants to US citizen siblings ages 5–18. The bottom figure compares the average residuals of regressing school enrollment rate on sex, disability status, English-speaking ability, and age, birth order, and state fixed effects for potentially DACA-eligible children ages 5–18 of undocumented immigrants to ineligible noncitizen siblings ages 5–18.

and $After_t$ are defined the same as above. The specification includes age and birth order fixed effects to account for the fact that citizens with undocumented parents tend to be younger than their noncitizen siblings. It also includes household fixed effects which control for any unobservable differences across families as well as state and year fixed effects, state-specific policies, state economic indicators, and observable family characteristics.

As before, the methodology's validity relies on parallel trends, which seem to hold, and the exogeneity of DACA. DACA-eligibility in this case is even more plausibly exogenous than in equation (3.1). Furthermore, netting out family specific unobservables reduces the likelihood of other factors, unrelated to DACA, confounding my results. While this sibling approach holds many advantages over other approaches, it does have the disadvantage of having a much smaller sample that is less representative of the overall population, as only households with at least on DACA-eligible sibling and at least one US citizen sibling in the relevant age range for a particular outcome are used in analysis.

3.4.2 Results

Table 3.1 presents the estimated effects of DACA on a litany of education-related outcomes by comparing all potentially DACA-eligible children to all citizen children with undocumented parents. Table 3.1 also displays the pre-2012 means of these outcomes for potentially eligible children to give the economic significance of these effects more context. As predicted, DACA incentivizes eligible youth to stay enrolled in school and ultimately graduate high school. School enrollment increases by 1.7 percentage points (pp), and high school graduation rates increase by 2.9 pp. These estimates are consistent with Kuka, Shenhav, and Shih (2018), though are somewhat smaller in magnitude. Part of this difference is likely due to me considering wider age ranges for

these outcomes.³⁸ When I restrict the age of my sample to match theirs, my estimate on enrollment does not change much at all, though the high school graduation coefficient does increase some. This might suggest that their proxy for undocumented children could be overstating the true impact of DACA by capturing some of the effects of children with more highly educated (and less likely to be undocumented) parents.³⁹

In addition to confirming the prediction that DACA increases enrollment and high school completion rates, I also find that DACA significantly increases school performance, at least according to one metric. DACA increases enrollment in the correct grade for a student's age by 3.4 pp. At the very least, DACA seems to encourage potentially eligible students to perform well enough to not be held back a grade. DACA also reduces the high school dropout rate by about 2.9 pp, though this is not surprising given the findings on enrollment and high school graduation. Interestingly, I find no effect on teenage pregnancy. This could partly be due to teenage pregnancy rates being particularly low among this group, even before DACA.

My results also point to DACA incentivizing increases in both college enrollment and employment. My estimate that employment increases by 3.4 pp is consistent with the prior DACA literature, though again, smaller in magnitude. My estimate on college enrollment is more interesting, however. I find that DACA increases college enrollment among potentially eligible individuals by 3.6 pp. This aligns with Kuka, Shenhav, and Shih (2018) and stands in contrast with other pa-

³⁸Kuka, Shenhav, and Shih (2018) focus on 14–18 year olds for enrollment and 19–22 year olds for high school graduation. My reasoning behind adjusting the age range is twofold. First, I believe it is very likely that younger students (or at least their parents) might be incentivized by the possibility of some day becoming DACA-eligible. Second, allowing individuals up to the age of 24 in my sample gives me a larger sample size for performing the sibling-comparison.

³⁹If the sample composition changes overtime to include more of these types of children, who, by virtue of their parents, have better educational outcomes despite being unlikely to be DACA-eligible, their point estimates will be biased upwards.

Table 3.1: The Effects of DACA (All Children with Undocumented Parents)

	enrolled in school (1)	enrolled in correct grade (2)	child born in last year (3)	dropped out school (4)	graduated high school (5)	enrolled in college (6)	employed (7)
$Eligible_i \times After_t$	0.017*** (0.003)	0.034*** (0.005)	-0.005 (0.005)	-0.029*** (0.005)	0.029*** (0.008)	0.036*** (0.009)	0.034*** (0.006)
$Eligible_i$	-0.025*** (0.002)	-0.050*** (0.003)	0.005* (0.003)	0.028*** (0.003)	-0.048*** (0.009)	-0.082*** (0.006)	-0.010 (0.008)
Observations	251,374	185,516	30,387	46,263	45,978	45,978	92,241
Pre-DACA mean	.926	.831	.037	.102	.706	.253	.391
Age range	5–18	5–18	15–18	16–18	19–24	19–24	16–24
Other restrictions		post 2008	girls only				

Notes: 2005 - 2016 ACS data restricted to children of likely undocumented immigrants who are either citizens or meet all of DACA's non-education criteria. $Eligible_i = 1$ if individual i is potentially eligible for DACA. $After_t = 1$ if the year is after 2012 (the year DACA was implemented). All observations from 2012 are excluded. Controls include individual covariates, state immigration policy indicators, state economic indicators, age fixed effects, state fixed effects, year fixed effects, and state-specific linear time trends. All regressions are weighted using the ACS person weights. Standard errors clustered at the state level in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

pers in the literature, such as Amuedo-Dorantes and Antman (2017) and Pope (2016), which find a negative or zero effect on college attendance. This seems to be mainly driven by the fact that these papers condition on individuals having completed high school. Even though DACA might provide strong enough job opportunities to encourage students who graduate high school to forgo college and begin working, more students in total are completing high school because of DACA and eligible to go to college. Thus, college enrollment, without first conditioning on high school graduation, is increasing. These results also suggest that some individuals might be working while attending college since both college enrollment and employment increase.⁴⁰

Table 3.2 shows results from the sibling comparison analysis. These findings are largely consistent with those in Table 3.1 with a couple of notable exceptions. First, while many of the

⁴⁰The average attendance and employment rates among potentially eligible youth do imply that this need not necessarily be the case, though.

Table 3.2: The Effects of DACA (Siblings with Different DACA Eligibility)

	enrolled in school (1)	enrolled in correct grade (2)	child born in last year (3)	dropped out school (4)	graduated high school (5)	enrolled in college (6)	employed (7)
$Eligible_i \times After_t$	0.024*** (0.006)	0.054*** (0.008)	-0.037 (0.058)	-0.169** (0.082)	0.068** (0.034)	-0.017 (0.040)	0.021 (0.023)
$Eligible_i$	-0.027*** (0.004)	-0.024*** (0.008)	-0.012 (0.067)	-0.159 (0.100)	-0.033 (0.024)	-0.036 (0.026)	-0.018 (0.017)
Observations	48,443	33,100	593	1,055	2,452	2,452	8,937
Pre-DACA mean	.928	.825	.028	.145	.746	.257	.503
Age range	5–18	5–18	15–18	16–18	19–24	19–24	16–24
Other restrictions		post 2008	girls only				

Notes: 2005 - 2016 ACS data restricted to children living in families with undocumented parents, at least one potentially DACA-eligible child, and at least one citizen child. $Eligible_i = 1$ if individual i is potentially eligible for DACA. $After_t = 1$ if the year is after 2012 (the year DACA was implemented). All observations from 2012 are excluded. Controls include individual covariates, age fixed effects, birth order fixed effects, and household (by state and year) fixed effects. All regressions are weighted using the ACS person weights. Standard errors clustered at the household level in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

point estimates are larger in magnitude using this strategy, the estimate on dropout rate is over five times the previous coefficient. This seems to be at least in part caused by the considerably small number of siblings between the ages of 16 and 18 with different DACA-eligibility statuses.⁴¹ Such a small sample size is unlikely to be representative. Second, under the sibling analysis, DACA has no effect on college attendance or employment. This might suggest that an individual's decision to attend college or start working has more to do with family influence or pressures than simply being eligible for DACA. This also might suggest that the composition of undocumented families in the US is changing over time.⁴²

⁴¹Indeed, the point estimate on teenage pregnancy also seems to be affected by this issue. If I allow the age range on these outcomes to extend to 24, the estimates become much more similar to those in Table 3.1.

⁴²In particular, policies such as SMYP and E-Verify mandates have been shown to cause undocumented immigrants to leave the US. If these immigrants are, for whatever reason, more opposed to college or less able to support their children financially past the age of 18, then changing composition could explain the differences in these particular findings between Table 3.1 and Table 3.2.

One more interesting detail from this analysis is that, following DACA, potentially-eligible individuals are essentially catching up to their citizen counterparts' level of education. In both Tables 3.1 and 3.2, the estimated impact of DACA is in many cases very close to being the reciprocal of the “penalty” for not being a citizen. Thus, at least according to these measures, DACA closes the education gap.

3.5 Part II: The Effects of State Immigration Reform

While DACA has a positive impact on young, eligible undocumented immigrants, it is far from the only policy that could have impacted this group's human capital decisions over the same time period. As mentioned above, several states implemented laws, such as SMYP and E-Verify mandates, designed to target undocumented immigrants. Even if these laws were designed primarily to affect adults, any impact on parents could influence their children's enrollment, school performance, graduation and other outcomes. The primary effect that SMYP and E-Verify mandates have is to reduce employment and hourly wages among undocumented men. In many undocumented families, the father is the main income earner. If his earning ability declines, it is possible that some undocumented mothers will enter the labor force to try supplement family income. This likely reduces the amount of parental interaction children in these families have. Even if mothers do not go to work, the increased family fear and stress could take a toll on children's education.

These immigration reforms might be predicted to reduce enrollment, enrollment in the correct grade for age, and graduation rates for children with undocumented parents for the reasons listed above and because these reforms reduce the returns to education.⁴³ Similarly, college en-

⁴³This is because their future earning potential decreases due to expected lower wages and greater difficulty finding employment.

rollment might also be expected to decrease. If these reforms lead to less interaction with parents, then teenage births might also be expected to increase. Employment among the children of undocumented immigrants could possibly rise or fall following these reforms. On the one hand, family financial difficulties might encourage teens to dropout of school and start working to supplement the family income.⁴⁴ On the other hand, these reforms make it more difficult for undocumented immigrants to find jobs. This effect could very well be different for children who are US citizens and those who are not. US citizens, even those with undocumented parents, should not have greater difficulty finding employment following these reforms.⁴⁵

Finally, while both SMYP and E-Verify mandates have similar effects on adults' labor market outcomes the different mechanisms through which they operate might lead to different effects on children. SMYP, by adding a much higher threat of deportation, might lead to higher family stress levels than E-Verify mandates. These deportations could also happen to any undocumented individual older than 14, so some children could be directly impacted. Thus, SMYP might be expected to have a larger impact on children with undocumented parents than E-Verify mandates. Furthermore, since it adds a further direct impact on undocumented children by reporting their families' presence to state officials, Alabama HB 56 could be expected to have a larger impact than other SMYP laws.⁴⁶

⁴⁴In order to test this as a particular mechanism, I restrict the sample to only 16–18 year olds when looking at employment as an outcome.

⁴⁵Evidence does point to these types of reforms dampening the economy in these states and reducing the employment rate among all low-skilled workers. Even so, it is much easier for a citizen to find a job than an undocumented immigrant. See Chapter 1.

⁴⁶Alabama HB 56 is also generally considered to be the strictest law against undocumented immigration in the US.

3.5.1 Methodology

To test these predictions, I utilize a difference-in-difference framework with the following estimating equation:

$$Y_{iast} = \alpha + \beta policy_{st} + \gamma X_{iast} + \delta Z_{st} + \mu_a + \mu_t + \mu_{st} + \epsilon_{iast} \quad (3.3)$$

As before the sample is restricted to only the children of undocumented immigrants. Y_{iast} is the outcome for child i who is a years old, living in state s and year t . X represents a vector of individual covariates, such as sex, English proficiency, having a recorded disability, household size, household income, having both parents present, and number of siblings. Z is a vector state-level variables including the state unemployment rate, log government spending per capita, and number of new housing permits to capture the business cycle. $policy_{st}$ takes a value of one if state s is implementing a particular immigration policy in year t . The policies I present results for are Alabama HB 56, SMYP enforcement, and Universal E-Verify mandates. The equation also includes age, state, and year fixed effects.

To examine how these policies affect children with different citizenship statuses differently, I run the regressions on two subsamples. The first is noncitizen children with undocumented parents. For this sample I add controls for the number of years they have been in the US and whether or not they are potentially eligible for DACA. The second group is citizen children with undocumented parents. As before, I weight all my regressions using the ACS person weights.

Proper identification relies on the policies being exogenous and the control group being comparable. In Chapter 1, I show that a variety of observable variables about states and their populations do not predict whether or not a state implements one of these laws. This increase the plausibility of the exogeneity assumption. As for the control group, I use children with the same

parental and citizenship status living in the non-treated states. To alleviate some concerns about differential pre-trends, I include state-specific linear time trends in my specification.

3.5.2 Results

The results of the state immigration policy analysis are displayed in Table 3.3. The top panel shows the effects of the three types of reforms on noncitizen children with undocumented parents, and the bottom panel shows the effects on citizen children with undocumented parents. I also include the pre-2008 means of the outcome variables for children in the treated states.⁴⁷

Interestingly, neither SMYP nor universal E-Verify mandates seem to have much impact on children at all. The few significant effects that do show up more or less align with the predicted effects. However, universal E-Verify mandates do seem to reduce employment for US citizens. This might be explainable in part by the large negative effects that this policy in particular has on state GDP. It is also possible that the most able citizens leave home once they are of working age. If this is more likely to happen after immigration reform, then my estimated employment effects will have a large negative bias.

Unlike SMYP and universal E-Verify mandates though, Alabama HB 56 has large impacts on undocumented children's education outcomes. As predicted, undocumented children are less likely to be enrolled and more likely to drop out of school. These numbers are quite large, a 7.8 pp drop in enrollment rate and a massive 25.6 pp increase in dropout rate. This fits with the anecdotal stories of Hispanic student enrollment dropping dramatically in Alabama immediately

⁴⁷I choose 2008 as the cutoff because the first Universal E-Verify mandate was enacted in 2008. SMYP laws were not enacted until later. This saves space, but unfortunately means that I cannot report a pre-Universal E-Verify mean for enrollment in the correct grade for age.

Table 3.3: The Effects of State Immigration Reform

	enrolled in school (1)	enrolled in correct grade (2)	child born in last year (3)	dropped out school (4)	graduated high school (5)	enrolled in college (6)	employed (7)
<i>Noncitizen children:</i>							
Alabama HB 56	-0.078*** (0.006)	-0.005 (0.010)	0.113*** (0.030)	0.256*** (0.012)	0.350*** (0.021)	-0.033 (0.021)	-0.254*** (0.016)
SMYP Enforced	-0.008 (0.013)	0.002 (0.032)	-0.181 (0.162)	-0.013 (0.030)	-0.021 (0.046)	-0.031 (0.020)	-0.014 (0.045)
Universal E-Verify Mandates	-0.015 (0.011)	-0.002 (0.032)	-0.125 (0.130)	0.053** (0.023)	0.033 (0.035)	-0.070*** (0.022)	0.024 (0.030)
Observations	62,844	41,781	12,390	20,345	27,859	27,859	20,345
Pre-2008 mean	.903	—	.038	.153	.594	.178	.243
<i>Citizen children:</i>							
Alabama HB 56	0.034*** (0.002)	-0.020*** (0.005)	0.073*** (0.020)	0.207*** (0.009)	0.217*** (0.020)	-0.038** (0.018)	0.185*** (0.020)
SMYP Enforced	-0.004 (0.006)	0.007* (0.004)	-0.052 (0.203)	-0.007 (0.027)	-0.046 (0.032)	-0.046 (0.032)	-0.042 (0.029)
Universal E-Verify Mandates	-0.002 (0.005)	-0.003 (0.009)	0.017 (0.104)	0.009 (0.023)	-0.022 (0.034)	-0.027 (0.029)	-0.068*** (0.022)
Observations	221,193	175,252	22,400	33,025	30,377	30,377	33,025
Pre-2008 mean	.962	—	.028	.060	.771	.344	.204
Age range	5–18	5–18	15–18	16–18	19–24	19–24	16–18
Other restrictions		post 2008	girls only				

Notes: 2005 - 2016 ACS data restricted to children with undocumented parents. Each cell represents a different regression measuring the effects of that specific immigration policy on the outcome variable. Results for children without citizenship are shown in the top portion of the table. Results for children with citizenship are shown in the bottom portion of the table. Controls include individual covariates, state economic indicators, age fixed effects, state fixed effects, year fixed effects, and state-specific linear time trends. All regressions are weighted using the ACS person weights. Standard errors clustered at the state level in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

following the bill's enforcement. Alabama HB 56 also greatly reduces employment among this group, which is no surprise given that the bill also included the strictest universal E-Verify mandate. Teenage births also increase by an astounding 11.8 pp. These lends credence to the idea that these teens might have less parental supervision or could be acting out due to stress following HB 56. Interestingly, high school graduation increases despite the decrease in enrollment and increase in dropouts. This could mean that undocumented immigrants return to complete their GED once they are unable to find a job. Alternatively, those without a high school degree might be incentivized to leave the state all together. Still, when taken as a whole, Alabama HB 56 has large negative impacts on undocumented children.

When looking at the effects of HB 56 on citizens with undocumented parents, it becomes apparent that the law is incentivizing these teenagers to drop out of school and become employed. They are also less likely to be enrolled in college, which is consistent with trading off education for immediate employment. Their citizenship, in theory, makes it easier for them to find jobs, which can help increase household income. Citizen children, like their noncitizen counterparts are also more likely to have teenage births following HB 56. They also see an increase in high school completion. Once again, this could be an increase in GEDs after working to help supplement family income or could signify a compositional change in the types of families living in Alabama. Unlike the noncitizen children, though, the citizen children actually increase enrollment in primary and secondary schools. This is likely to be driven by younger citizen children and could possibly be evidence that families with citizen children are either less concerned by the head count requirement in HB 56 or that if families are worried enough to pull their citizen children out of school, they are leaving Alabama all together.

These results for Alabama should be treated somewhat cautiously, though. After all, Al-

Alabama is just a single state with a relatively small population of undocumented immigrants. Some of these findings could simply be statistical noise driven by a few observations. To alleviate some of this concern, I can perform the same analysis with groups of children who are unlikely to be impacted much by HB 56: children of Hispanic citizens and children of non-Hispanic citizens.⁴⁸ This analysis finds much smaller effects that are practically zero in many cases. Even so, there are still some statistically significant coefficients that suggest that more might have been going on in Alabama than simply HB 56 during this time period. Still, the magnitudes of my estimates for the children of undocumented immigrants likely indicate that HB 56 had at least some negative impact on them.

3.6 Conclusion

Throughout this paper, I have provided estimates of how different types of immigration policies affect the children of undocumented immigrants. Apart from a recent surge in literature related to DACA, this group has remained understudied. This paper contributes not only by studying undocumented children in a new policy setting, but also by refining the method of identifying undocumented children and finding a more similar control group. By comparing outcomes of potentially DACA-eligible children with their citizen siblings, I find that DACA increases enrollment among 5–18 year olds by 2.4 percentage points and increases their high school graduation rate by nearly 10%. DACA also plausibly increases school performance for these children by reducing the percentage that have been held back a grade. While the sibling analysis does not find much evidence of DACA affecting college and employment decisions, similar analysis using all children

⁴⁸To keep their parents as similar as possible to undocumented parents, I focus on children of low-skilled parents in both cases.

of undocumented immigrants suggests that DACA might increase both the college enrollment rate and employment rate of eligible individuals. All in all, DACA closes the education gap between undocumented children and their citizen counterparts. With all the positive externalities associated with having a more highly educated society, DACA can be seen as a net positive in this regard.

Just as DACA has been a boon for immigrant education, other types of immigration reform have had the opposite effect. In particular, Alabama HB 56, which in addition to imposing SMYP and universal E-Verify mandates on undocumented parents, also requires schools to send reports to the state that estimate the number of unauthorized immigrants attending, has a large negative impact on these children's schooling. Noncitizen children of undocumented immigrants seem to be dropping out of school and not finding employment. Meanwhile, citizen children seem to be dropping out of school to work, which may be in order to help supplement the family income. Both types of children also see large increases in teenage births, which can be a further detriment to their human capital accumulation.

These results have great significance as the debate over immigration policy continues in the United States. With DACA currently being suspended, it is important to understand exactly how and by how much it improved the economic outlook for its recipients. These individual gains are likely to lead to societal gains as well. As for tough state policies against undocumented immigration, my findings illustrate that the costs of these reforms can extend far beyond just the adults that they target. The majority of the children of undocumented immigrants in the United States are in fact US citizens, and even they are potentially being held back by these policies. These costs should not be ignored as these policies and many others like them are debated in other states in the future.

Appendices

Appendix A

Appendix Tables and Figures

Table A.1: States with E-Verify Laws (2005-2015)

State	Public E-Verify Effective Date	Universal E-Verify Effective Date
Alabama	April, 2012	April, 2012
Arizona	January, 2008	January, 2008
Colorado	August, 2006	
Florida	January, 2011	
Georgia	July, 2007	January, 2012
Idaho	July, 2009	
Indiana	July, 2011	
Louisiana	January, 2012	
Minnesota	January, 2008	
Mississippi	July, 2008	July, 2008
Missouri	January, 2009	
Nebraska	October 2009	
North Carolina	January, 2007	October, 2012
Oklahoma	November, 2007	
Rhode Island	March, 2008*	
South Carolina	January, 2009	January, 2012
Tennessee	January, 2012	January, 2012
Utah	July, 2009	July, 2010
Virginia	June, 2011	
West Virginia	June, 2012	

*Rhode Island ended its mandatory E-Verify laws in 2011.

Table A.2: States with SMYP Laws (2005-2015)

	Alabama	Arizona	Georgia	Indiana	South Carolina	Utah
Law Name	HB 56	SB 1070	HB 87	SB 590	SB 20	HB 497
Date Passed	6/9/2011	4/23/2010	5/13/2011	5/10/2011	6/27/2011	3/15/2011
Date Supposed to be Enacted	8/1/2011	7/29/2010	7/1/2011	7/1/2011	1/1/2012	7/1/2011
Date Actually Enacted*	9/29/2011	6/25/2012	8/1/2012**	Never	11/15/2012	Never

*The date law officers were required to make a reasonable attempt to determine a person's legal status if they have reasonable suspicion they may not be in the country lawfully

**Many of Georgia's largest law enforcement agencies, including the Atlanta Police Department opted not to enforce HB 87

Table A.3: Industries of Employment

	NCI Hispanic (1)	naturalized Hispanic (2)	US-born Hispanic (3)	US-born non-Hispanic (4)
<i>men:</i>				
agriculture	0.085	0.032	0.019	0.029
mining	0.006	0.007	0.013	0.012
utilities	0.003	0.007	0.011	0.016
construction	0.275	0.167	0.126	0.146
manufacturing	0.135	0.166	0.115	0.166
wholesale trade	0.037	0.049	0.038	0.038
retail trade	0.069	0.094	0.142	0.124
transportation services	0.039	0.093	0.066	0.068
information services	0.006	0.013	0.020	0.020
financial services	0.017	0.040	0.038	0.032
professional and business services	0.117	0.092	0.092	0.080
educational services	0.009	0.025	0.033	0.030
health care services	0.009	0.028	0.033	0.029
entertainment and food services	0.129	0.097	0.122	0.095
other services	0.051	0.052	0.045	0.044
government	0.004	0.023	0.046	0.046
<i>women:</i>				
agriculture	0.058	0.014	0.008	0.008
mining	0.000	0.001	0.002	0.002
utilities	0.001	0.002	0.004	0.004
construction	0.012	0.012	0.013	0.018
manufacturing	0.150	0.123	0.059	0.078
wholesale trade	0.037	0.030	0.020	0.018
retail trade	0.109	0.126	0.175	0.160
transportation services	0.016	0.031	0.025	0.027
information services	0.007	0.012	0.018	0.018
financial services	0.026	0.068	0.077	0.079
professional and business services	0.114	0.095	0.085	0.082
educational services	0.034	0.088	0.085	0.080
health care services	0.067	0.151	0.158	0.177
entertainment and food services	0.213	0.112	0.149	0.134
other services	0.120	0.091	0.050	0.057
government	0.008	0.029	0.044	0.044

Notes: 2005 - 2015 American Community Survey (ACS) data. The sample is restricted to low-skilled adults from ages of 16-64. NCI stands for "non-citizen, immigrant," and is the group used to proxy for undocumented immigrants.

Table A.4: Industry Descriptions

Industry	Examples
agriculture	crop production animal production and aquaculture logging
mining	oil and gas extraction coal mining
utilities	electric power generation, transmission and distribution sewage treatment facilities water, steam, air conditioning, and irrigation systems
construction	construction, including cleaning during and immediately after
manufacturing	retail bakeries textile product mills pharmaceuticals and medicines
wholesale trade	motor vehicle and motor vehicle parts and supplies merchant wholesalers lumber and other construction materials merchant wholesalers alcoholic beverages merchant wholesalers
retail trade	automobile dealers department and discount stores grocery stores
transportation services	bus service and urban transit air transportation warehousing and storage
information services	newspaper publishers wired telecommunications carriers data processing, hosting, and related services
financial services	banking and related activities insurance carriers and related activities real estate
professional and business services	accounting, tax preparation, bookkeeping, and payroll services services to buildings and dwellings landscaping services
educational services	elementary and secondary schools colleges, universities and professional schools business, technical, and trade schools and training
health care services	nursing care facilities hospitals child day care services
entertainment and food services	performing arts, spectator sports, and related industries museums, art galleries, historical sites, and similar institutions restaurants and other food services
other services	automotive repair and maintenance barber shops and beauty salons private households
government	executive offices and legislative bodies justice, public order, and safety activities military

Source: 2015 ACS PUMS Data Dictionary

Table A.5: Exogeneity of the Policies

	SMYP (1)	universal E-Verify (2)	public E-Verify (3)
NCI Hispanic	0.004 (0.012)	0.008 (0.023)	-0.010 (0.022)
naturalized Hispanic	-0.001 (0.012)	-0.012 (0.021)	0.028 (0.045)
US-born Hispanic	0.007 (0.017)	0.002 (0.029)	-0.015 (0.027)
black	0.015 (0.014)	0.033 (0.023)	0.002 (0.021)
female	0.000 (0.000)	0.000 (0.000)	-0.002** (0.001)
age	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
high school degree	-0.001 (0.002)	-0.002 (0.003)	0.007 (0.006)
Ability to speak English	0.003 (0.002)	0.008** (0.004)	0.004 (0.006)
disabled	0.001 (0.002)	0.002 (0.003)	-0.008 (0.006)
married	-0.001 (0.003)	-0.000 (0.005)	-0.010 (0.008)
children	-0.000 (0.001)	0.002 (0.002)	-0.001 (0.002)
state unemployment rate	0.651 (0.440)	1.440 (0.871)	0.885 (1.499)
log state expenditures per capita	-0.057 (0.048)	-0.154 (0.101)	-0.366** (0.179)
state housing permits	-0.000 (0.000)	-0.000* (0.000)	-0.000*** (0.000)
Observations	14,161,017	14,161,017	14,161,017
Prob > F	.996	.243	0.031

Notes: 2005 - 2015 American Community Survey (ACS) data. Prob > F reports the p-value on the test of joint significant of all the control variables. Standard errors clustered at the state level in parentheses: *** p < 0.01, ** p < 0.05, * p < 0.1

Table A.6: Employment and Wage Results (Omitting SMYP Treatment)

	men		women	
	employment (1)	log wage (2)	employment (3)	log wage (4)
<i>NCI Hispanics:</i>				
universal E-Verify	-0.038*** (0.013)	-0.031*** (0.011)	-0.007 (0.005)	0.026** (0.011)
public E-Verify	-0.019*** (0.005)	-0.020** (0.010)	-0.009** (0.004)	-0.017 (0.014)
Observations	425,261	340,942	366,626	180,095
<i>naturalized Hispanics:</i>				
universal E-Verify	-0.008* (0.004)	-0.004 (0.018)	-0.014 (0.011)	0.037* (0.019)
public E-Verify	-0.005 (0.006)	-0.006 (0.009)	0.004 (0.004)	-0.005 (0.011)
Observations	179,303	141,641	194,053	123,472
<i>US-born Hispanics:</i>				
universal E-Verify	-0.020*** (0.004)	-0.006 (0.013)	-0.002 (0.008)	0.000 (0.012)
public E-Verify	-0.007 (0.004)	-0.005 (0.013)	-0.002 (0.003)	-0.003 (0.010)
Observations	635,958	419,018	623,855	385,069
<i>US-born non-Hispanics:</i>				
universal E-Verify	-0.006*** (0.002)	0.003 (0.007)	0.000 (0.002)	-0.000 (0.006)
public E-Verify	-0.002 (0.002)	0.000 (0.006)	-0.001 (0.001)	0.004 (0.005)
Observations	5,898,023	4,142,344	5,837,938	3,861,050
Year FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓

Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The coefficients on employment measure the percentage point change in the number employed divided by the total population (including those not in the labor force). The model controls for individual level characteristics and state level business cycle variables. Standard errors clustered at the state level in parentheses: *** p < 0.01, ** p < 0.05, * p < 0.1

Table A.7: Detailed Employment Results

	men			women		
	employment (1)	unemployment (2)	labor force participation (3)	employment (4)	unemployment (5)	labor force participation (6)
<i>NCI Hispanics:</i>						
SMYP	-0.018** (0.008)	-0.001 (0.004)	-0.020*** (0.006)	0.001 (0.004)	0.000 (0.003)	0.002 (0.003)
universal E-Verify	-0.034*** (0.011)	0.006** (0.002)	-0.028*** (0.010)	-0.007 (0.005)	-0.002 (0.003)	-0.009 (0.006)
public E-Verify	-0.020*** (0.005)	0.007* (0.004)	-0.013** (0.005)	-0.009** (0.004)	0.005 (0.003)	-0.004 (0.005)
Observations	425,261	425,261	425,261	366,626	366,626	366,626
<i>naturalized Hispanics:</i>						
SMYP	0.016 (0.015)	-0.008** (0.004)	0.008 (0.012)	0.012 (0.016)	-0.005 (0.009)	0.007 (0.008)
universal E-Verify	-0.013* (0.007)	0.007 (0.006)	-0.006 (0.004)	-0.017 (0.013)	0.002 (0.010)	-0.016* (0.008)
public E-Verify	-0.004 (0.006)	-0.001 (0.004)	-0.005 (0.004)	0.005 (0.003)	-0.007** (0.003)	-0.002 (0.004)
Observations	179,303	179,303	179,303	194,053	194,053	194,053
<i>US-born Hispanics:</i>						
SMYP	-0.000 (0.003)	-0.001 (0.003)	-0.001 (0.002)	-0.008** (0.003)	-0.005** (0.002)	-0.013*** (0.003)
universal E-Verify	-0.020*** (0.004)	0.012*** (0.003)	-0.008*** (0.003)	0.000 (0.008)	0.006 (0.004)	0.007 (0.005)
public E-Verify	-0.007 (0.004)	-0.002 (0.002)	-0.009** (0.003)	-0.003 (0.003)	-0.003 (0.002)	-0.006** (0.003)
Observations	635,958	635,958	635,958	623,855	623,855	623,855
<i>US-born non-Hispanics:</i>						
SMYP	-0.005*** (0.001)	0.002 (0.002)	-0.003 (0.003)	-0.001 (0.002)	0.000 (0.001)	-0.001 (0.002)
universal E-Verify	-0.005** (0.002)	0.001 (0.002)	-0.004*** (0.001)	0.000 (0.002)	-0.001 (0.001)	-0.001 (0.001)
public E-Verify	-0.002 (0.002)	0.000 (0.002)	-0.002** (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.002* (0.001)
Observations	5,898,023	5,898,023	5,898,023	5,837,938	5,837,938	5,837,938
Year FE	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓	✓	✓

Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The coefficients on employment measure the percentage point change in the number employed divided by the total population (including those not in the labor force). The model controls for individual level characteristics and state level business cycle variables. Standard errors clustered at the state level in parentheses: *** p < 0.01, ** p < 0.05, * p < 0.1

Table A.8: Migration Results

	men		women	
	migrated to state (1)	migrated from state (2)	migrated to state (3)	migrated from state (4)
<i>NCI Hispanics:</i>				
SMYP	-0.006 (0.009)	-0.005 (0.014)	-0.004 (0.003)	-0.009 (0.014)
universal E-Verify	-0.020*** (0.007)	0.002 (0.011)	-0.019*** (0.005)	0.002 (0.011)
public E-Verify	-0.012*** (0.002)	-0.012 (0.012)	-0.007* (0.003)	-0.008 (0.012)
Observations	425,261	425,261	366,626	366,626
<i>naturalized Hispanics:</i>				
SMYP	0.000 (0.007)	-0.004 (0.022)	-0.001 (0.003)	-0.015 (0.018)
universal E-Verify	-0.015*** (0.005)	-0.003 (0.015)	-0.012*** (0.004)	-0.004 (0.017)
public E-Verify	-0.009** (0.003)	-0.021 (0.020)	-0.005* (0.003)	-0.028 (0.020)
Observations	179,303	179,303	194,053	194,053
<i>US-born Hispanics:</i>				
SMYP	0.003 (0.002)	-0.013 (0.030)	0.002 (0.002)	-0.016 (0.030)
universal E-Verify	-0.014*** (0.004)	0.002 (0.016)	-0.016*** (0.005)	0.003 (0.016)
public E-Verify	-0.007* (0.004)	-0.017 (0.022)	-0.004** (0.002)	-0.016 (0.022)
Observations	635,958	635,958	623,855	623,855
<i>US-born non-Hispanics:</i>				
SMYP	-0.001 (0.002)	-0.001 (0.014)	-0.001 (0.001)	-0.006 (0.015)
universal E-Verify	-0.003* (0.001)	-0.005 (0.009)	-0.002** (0.001)	-0.006 (0.010)
public E-Verify	-0.001 (0.001)	-0.005 (0.009)	-0.000 (0.001)	-0.007 (0.009)
Observations	5,898,023	5,898,023	5,837,938	5,837,938
Year FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓

Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The coefficients on migrated to/from state in the last year measure the percentage point change in the number of people who have migrated to/from a state within the previous 12 months divided by the total population in that state. The model controls for individual level characteristics and state level business cycle variables. Hispanic types are non-citizen immigrant (NCI), citizen immigrant (CI), and native. Standard errors clustered at the state level in parentheses: *** p < 0.01, ** p < 0.05, * p < 0.1

Table A.9: Changing the Definition of NCI Hispanics

	men		women	
	employment (1)	log wage (2)	employment (3)	log wage (4)
<i>original definition:</i>				
SMYP	-0.018** (0.008)	-0.017* (0.009)	0.001 (0.004)	0.015 (0.016)
universal E-Verify	-0.034*** (0.011)	-0.027** (0.012)	-0.007 (0.005)	0.023* (0.011)
public E-Verify	-0.020*** (0.005)	-0.020** (0.009)	-0.009** (0.004)	-0.017 (0.014)
Observations	425,261	340,942	366,626	180,095
<i>restricting NCI Hispanics to immigrants from Mexico or Central America:</i>				
SMYP	-0.021** (0.009)	-0.013 (0.009)	0.002 (0.004)	0.023 (0.020)
universal E-Verify	-0.034*** (0.011)	-0.030** (0.013)	-0.007 (0.005)	0.020 (0.014)
public E-Verify	-0.018*** (0.005)	-0.019* (0.010)	-0.009* (0.005)	-0.028* (0.016)
Observations	382,629	309,438	320,022	153,773
<i>restricting NCI Hispanics to immigrants from Mexico only:</i>				
SMYP	-0.022* (0.012)	-0.009 (0.009)	-0.002 (0.005)	0.027 (0.019)
universal E-Verify	-0.038*** (0.010)	-0.033** (0.013)	-0.004 (0.007)	0.019 (0.014)
public E-Verify	-0.025*** (0.005)	-0.013 (0.013)	-0.006 (0.006)	-0.017 (0.013)
Observations	317,457	256,403	268,236	125,260
Year FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓

Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The original definition of NCI Hispanics is all non-citizen Hispanic immigrants with a high school education or less except those from Cuba or Puerto Rico. The coefficients on employment measure the percentage point change in the number employed divided by the total population (including those not in the labor force). The model controls for individual level characteristics and state level business cycle variables. Standard errors clustered at the state level in parentheses: *** p < 0.01, ** p < 0.05, * p < 0.1

Table A.10: Controlling for Other Treatments (NCI Hispanic)

	men		women	
	employment (1)	log wage (2)	employment (3)	log wage (4)
<i>main specification:</i>				
SMYP	-0.018** (0.008)	-0.017* (0.009)	0.001 (0.004)	0.015 (0.016)
universal E-Verify	-0.034*** (0.011)	-0.027** (0.012)	-0.007 (0.005)	0.023* (0.011)
public E-Verify	-0.020*** (0.005)	-0.020** (0.009)	-0.009** (0.004)	-0.017 (0.014)
Observations	425,261	340,942	366,626	180,095
<i>adding in separate treatment for passing SMYP without enforcement:</i>				
SMYP enforced	-0.015** (0.006)	-0.026 (0.019)	-0.000 (0.006)	0.011 (0.014)
SMYP passed	-0.005 (0.008)	0.013 (0.024)	0.002 (0.011)	0.006 (0.021)
universal E-Verify	-0.032*** (0.010)	-0.030** (0.015)	-0.008 (0.007)	0.021 (0.016)
public E-Verify	-0.019*** (0.005)	-0.021** (0.009)	-0.010** (0.004)	-0.018 (0.014)
Observations	425,261	340,942	366,626	180,095
<i>adding in separate treatment for statewide 287(g) agreements:</i>				
SMYP	-0.018** (0.008)	-0.017** (0.008)	0.001 (0.004)	0.015 (0.016)
universal E-Verify	-0.034*** (0.011)	-0.027** (0.012)	-0.007 (0.005)	0.023** (0.011)
public E-Verify	-0.020*** (0.005)	-0.020** (0.009)	-0.008** (0.004)	-0.016 (0.014)
statewide 287(g)	0.001 (0.010)	-0.006 (0.012)	-0.020*** (0.007)	-0.012 (0.026)
Observations	425,261	340,942	366,626	180,095
Year FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓

Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The coefficients on employment measure the percentage point change in the number employed divided by the total population (including those not in the labor force). The model controls for individual level characteristics and state level business cycle variables. Standard errors clustered at the state level in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.11: Changing the Timing of Treatment (NCI Hispanic)

	men		women	
	employment (1)	log wage (2)	employment (3)	log wage (4)
<i>main specification:</i>				
SMYP	-0.018** (0.008)	-0.017* (0.009)	0.001 (0.004)	0.015 (0.016)
universal E-Verify	-0.034*** (0.011)	-0.027** (0.012)	-0.007 (0.005)	0.023* (0.011)
public E-Verify	-0.020*** (0.005)	-0.020** (0.009)	-0.009** (0.004)	-0.017 (0.014)
Observations	425,261	340,942	366,626	180,095
<i>all partially treated years = control years:</i>				
SMYP	-0.002 (0.010)	0.009 (0.010)	-0.002 (0.006)	-0.015 (0.016)
universal E-Verify	-0.039** (0.017)	-0.044*** (0.013)	-0.001 (0.006)	0.027** (0.013)
public E-Verify	-0.012** (0.005)	-0.023*** (0.008)	-0.009* (0.005)	-0.014 (0.015)
Observations	425,261	340,942	366,626	180,095
<i>treatment date rounded to nearest January 1:</i>				
SMYP	-0.023*** (0.005)	-0.007 (0.009)	-0.005 (0.004)	0.004 (0.009)
universal E-Verify	-0.033*** (0.011)	-0.032** (0.012)	-0.002 (0.005)	0.024** (0.011)
public E-Verify	-0.019*** (0.005)	-0.013* (0.007)	-0.010** (0.004)	-0.021 (0.014)
Observations	425,261	340,942	366,626	180,095
<i>treatment years weighted by fraction of months treated:</i>				
SMYP	-0.017** (0.006)	-0.005 (0.010)	-0.002 (0.004)	-0.001 (0.014)
universal E-Verify	-0.035*** (0.013)	-0.033*** (0.012)	-0.004 (0.006)	0.029*** (0.011)
public E-Verify	-0.018*** (0.005)	-0.016* (0.009)	-0.010** (0.005)	-0.018 (0.013)
Observations	425,261	340,942	366,626	180,095
Year FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓

Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The coefficients on employment measure the percentage point change in the number employed divided by the total population (including those not in the labor force). The model controls for individual level characteristics and state level business cycle variables. Standard errors clustered at the state level in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.12: Changing the Age of the Sample (NCI Hispanic)

	men		women	
	employment (1)	log wage (2)	employment (3)	log wage (4)
<i>age 16 - 64:</i>				
SMYP	-0.018** (0.008)	-0.017* (0.009)	0.001 (0.004)	0.015 (0.016)
universal E-Verify	-0.034*** (0.011)	-0.027** (0.012)	-0.007 (0.005)	0.023* (0.011)
public E-Verify	-0.020*** (0.005)	-0.020** (0.009)	-0.009** (0.004)	-0.017 (0.014)
Observations	425,261	340,942	366,626	180,095
<i>age 20 - 64:</i>				
SMYP	-0.020** (0.010)	-0.018* (0.009)	0.002 (0.004)	0.018 (0.016)
universal E-Verify	-0.033*** (0.012)	-0.021* (0.012)	-0.007 (0.005)	0.024** (0.010)
public E-Verify	-0.019*** (0.005)	-0.018* (0.009)	-0.009* (0.005)	-0.017 (0.013)
Observations	398,090	328,979	345,208	173,928
<i>age 20 - 45:</i>				
SMYP	-0.027** (0.012)	-0.029** (0.012)	0.005 (0.008)	0.029 (0.017)
universal E-Verify	-0.033** (0.013)	-0.021* (0.011)	-0.007 (0.005)	0.030*** (0.011)
public E-Verify	-0.021*** (0.006)	-0.017 (0.010)	-0.012** (0.005)	-0.027** (0.013)
Observations	304,737	257,921	251,619	129,054
Year FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓

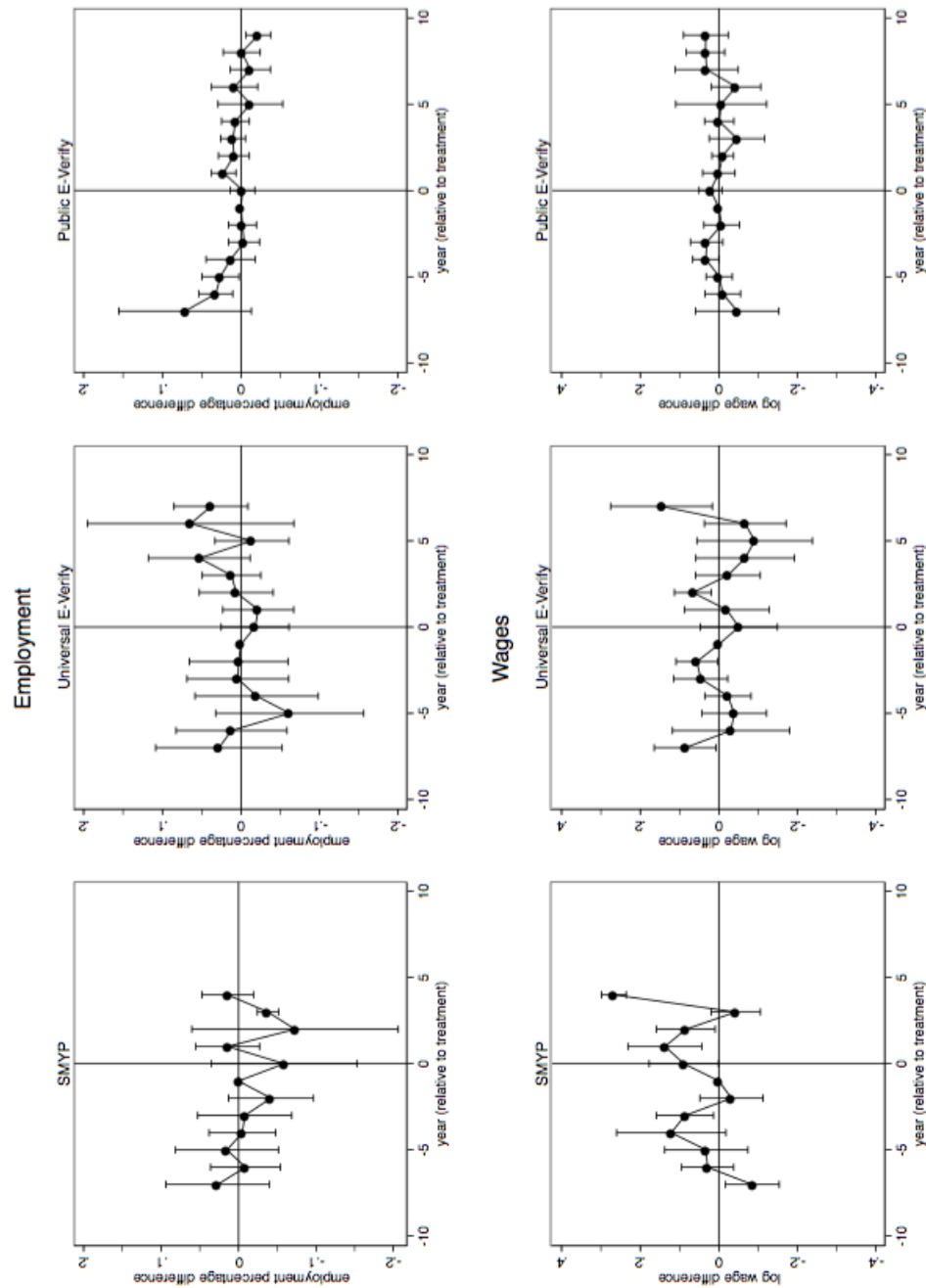
Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The coefficients on employment measure the percentage point change in the number employed divided by the total population (including those not in the labor force). The model controls for individual level characteristics and state level business cycle variables. Standard errors clustered at the state level in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.13: Other Robustness Tests (NCI Hispanic)

	men		women	
	employment (1)	log wage (2)	employment (3)	log wage (4)
<i>main specification:</i>				
SMYP	-0.018** (0.008)	-0.017* (0.009)	0.001 (0.004)	0.015 (0.016)
universal E-Verify	-0.034*** (0.011)	-0.027** (0.012)	-0.007 (0.005)	0.023* (0.011)
public E-Verify	-0.020*** (0.005)	-0.020** (0.009)	-0.009** (0.004)	-0.017 (0.014)
Observations	425,261	340,942	366,626	180,095
<i>including observations from Georgia:</i>				
SMYP	-0.012 (0.008)	0.000 (0.016)	0.002 (0.003)	0.000 (0.014)
universal E-Verify	-0.032*** (0.010)	-0.025** (0.012)	-0.007 (0.005)	0.019* (0.011)
public E-Verify	-0.020*** (0.005)	-0.026** (0.010)	-0.010** (0.004)	-0.014 (0.013)
Observations	437,294	350,875	374,923	184,064
<i>unweighted:</i>				
SMYP	-0.031*** (0.008)	-0.034*** (0.010)	0.005 (0.003)	0.021** (0.009)
universal E-Verify	-0.041*** (0.014)	-0.022* (0.011)	-0.006 (0.005)	0.011 (0.011)
public E-Verify	-0.017*** (0.005)	-0.019*** (0.006)	-0.007* (0.004)	-0.007 (0.012)
Observations	425,261	340,942	366,626	180,095
<i>excluding state business cycle variables:</i>				
SMYP	-0.009 (0.009)	-0.027*** (0.008)	0.004 (0.004)	-0.002 (0.019)
universal E-Verify	-0.030** (0.015)	-0.030** (0.014)	-0.006 (0.005)	0.014 (0.016)
public E-Verify	-0.014*** (0.004)	-0.025** (0.009)	-0.008* (0.004)	-0.027** (0.013)
Observations	425,848	341,441	367,097	180,416
Year FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓

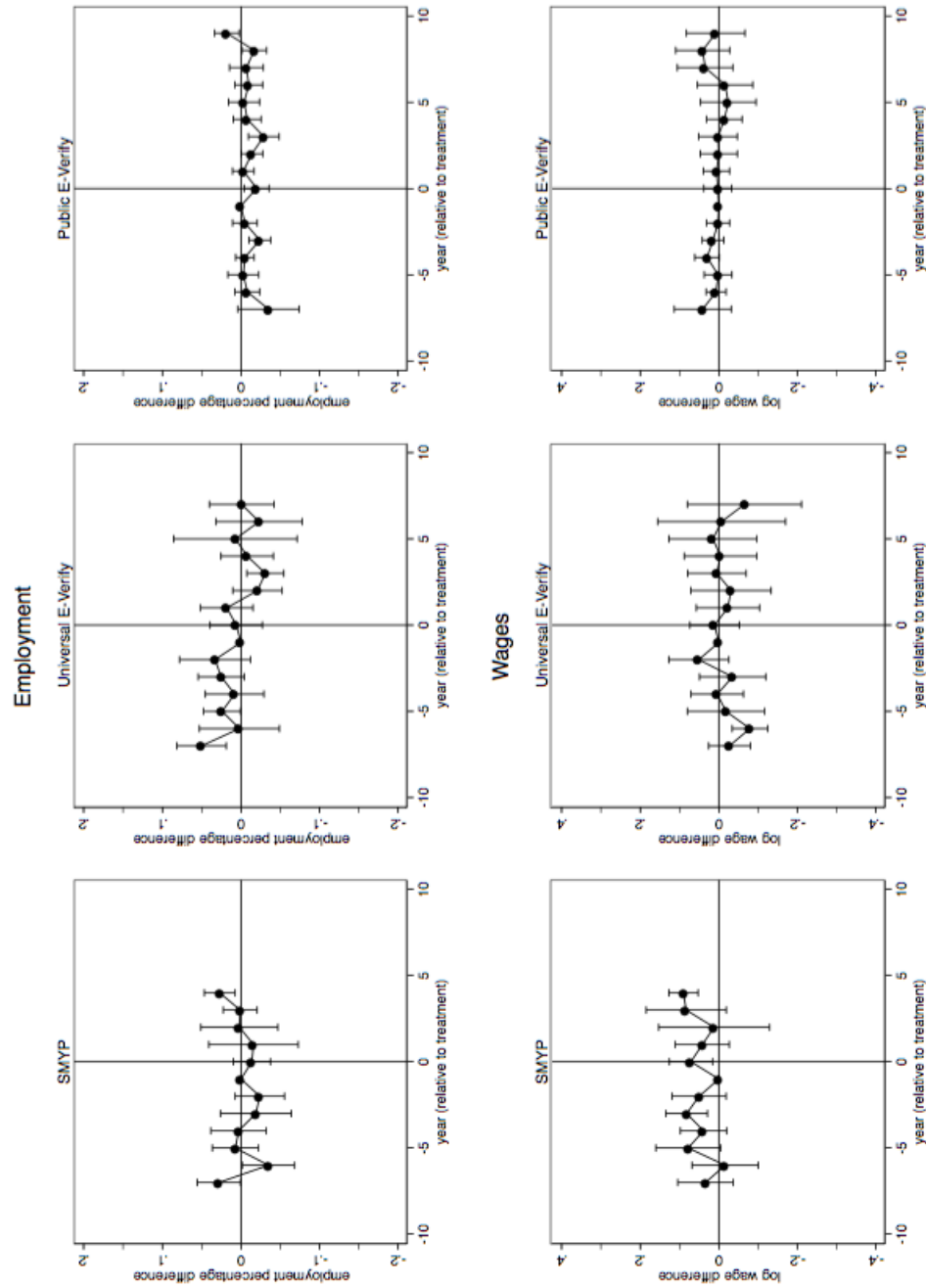
Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The coefficients on employment measure the percentage point change in the number employed divided by the total population (including those not in the labor force). The model controls for individual level characteristics and state level business cycle variables, except for the fourth panel. Standard errors clustered at the state level in parentheses: *** p < 0.01, ** p < 0.05, * p < 0.1

Figure A.1: Naturalized Hispanic Dynamic Effects (Men)



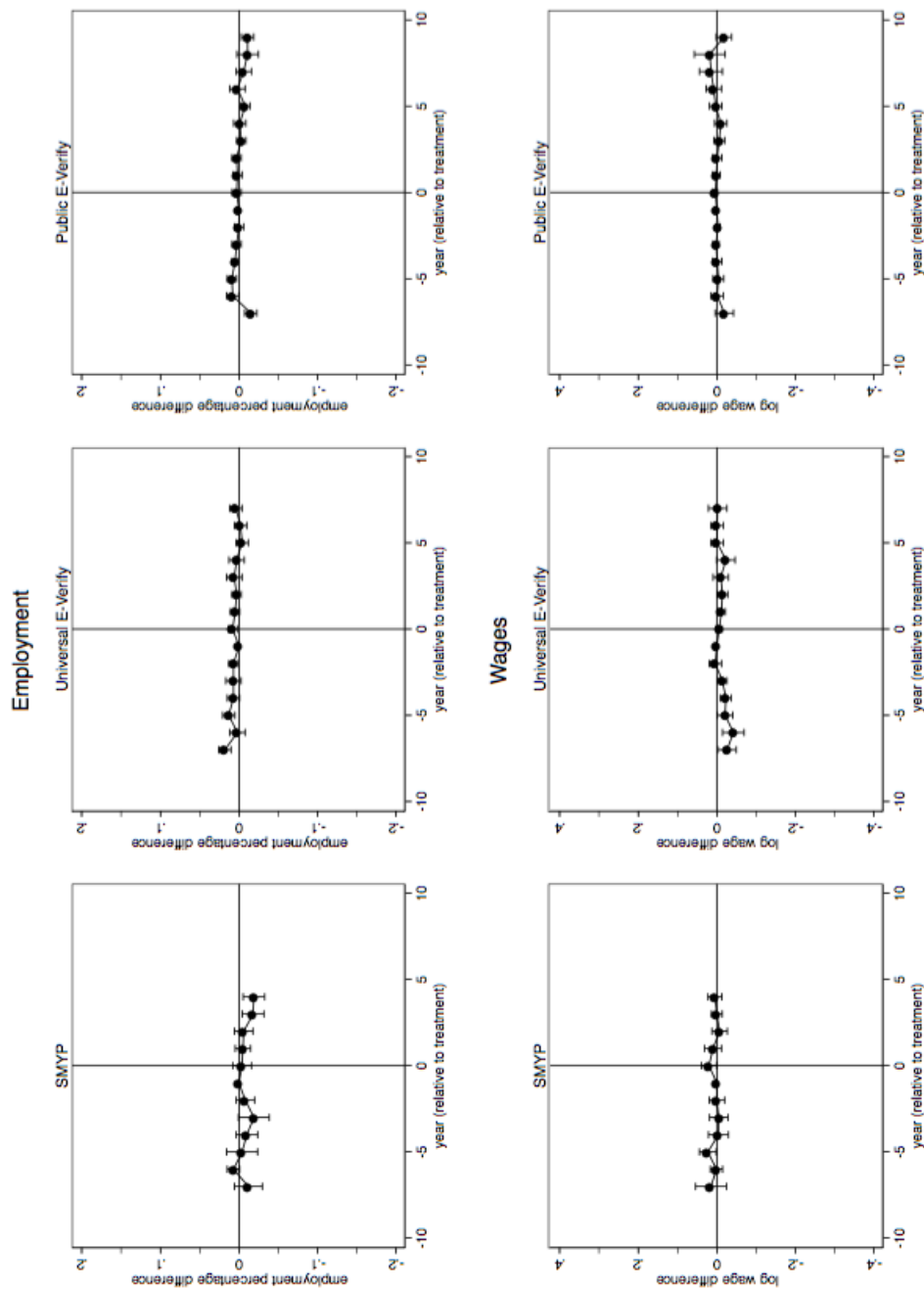
Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The sample is restricted to low-skilled, naturalized Hispanic men from ages of 16-64. The graphs plot the differences in the estimated effects of SMYP, universal E-Verify mandates, and public E-Verify mandates on employment percentage and log real hourly wages between individuals in the affected states and control states relative to a law implementation year of 0. These estimates net out state and year fixed effects. See section 5.3 for more details.

Figure A.2: US-born Hispanic Dynamic Effects (Men)



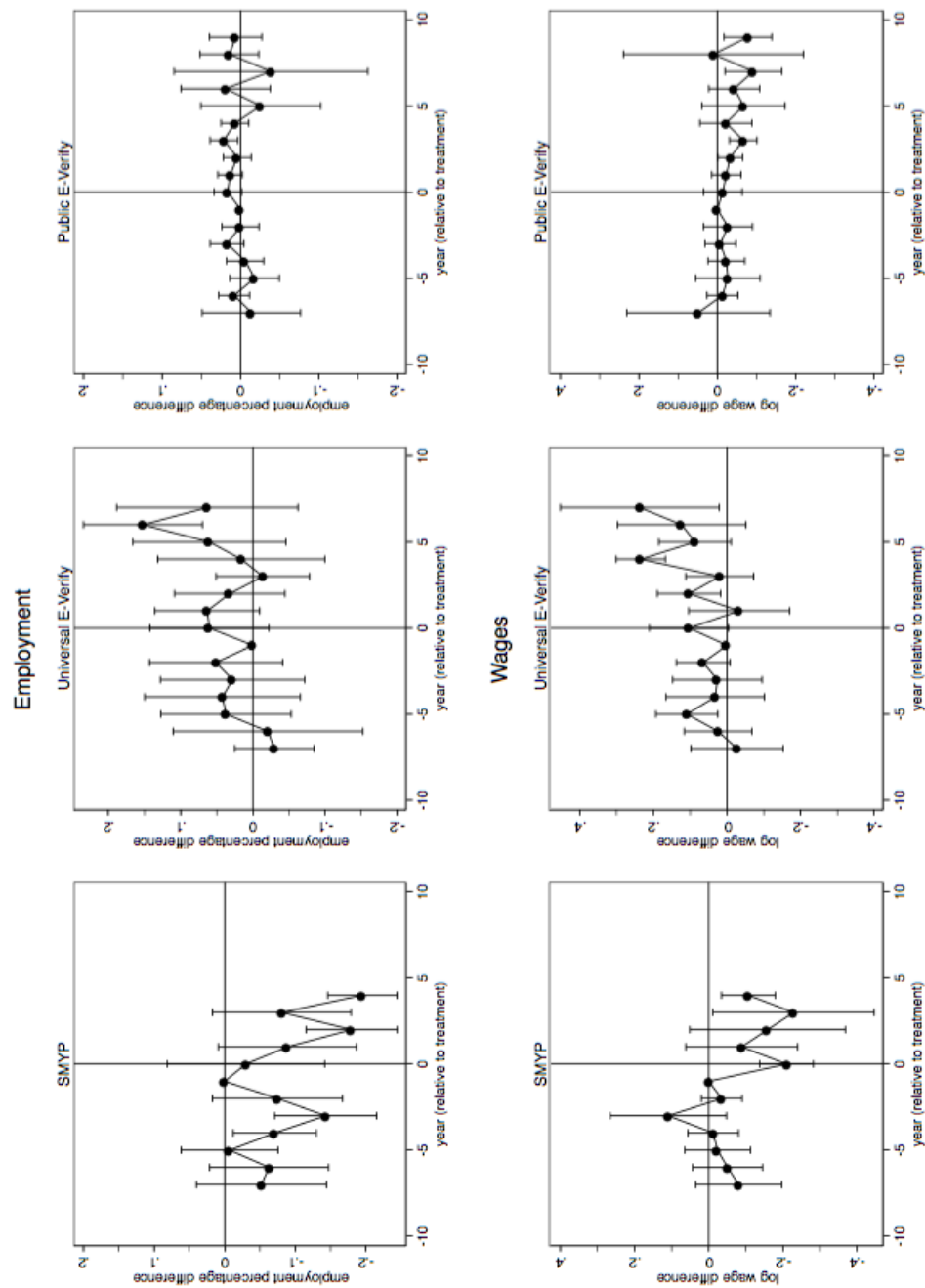
Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The sample is restricted to low-skilled, US-born Hispanic men from ages of 16-64. The graphs plot the differences in the estimated effects of SMYP, universal E-Verify mandates, and public E-Verify mandates on employment percentage and log real hourly wages between individuals in the affected states and control states relative to a law implementation year of 0. These estimates net out state and year fixed effects. See section 5.3 for more details.

Figure A.3: US-born non-Hispanic Dynamic Effects (Men)



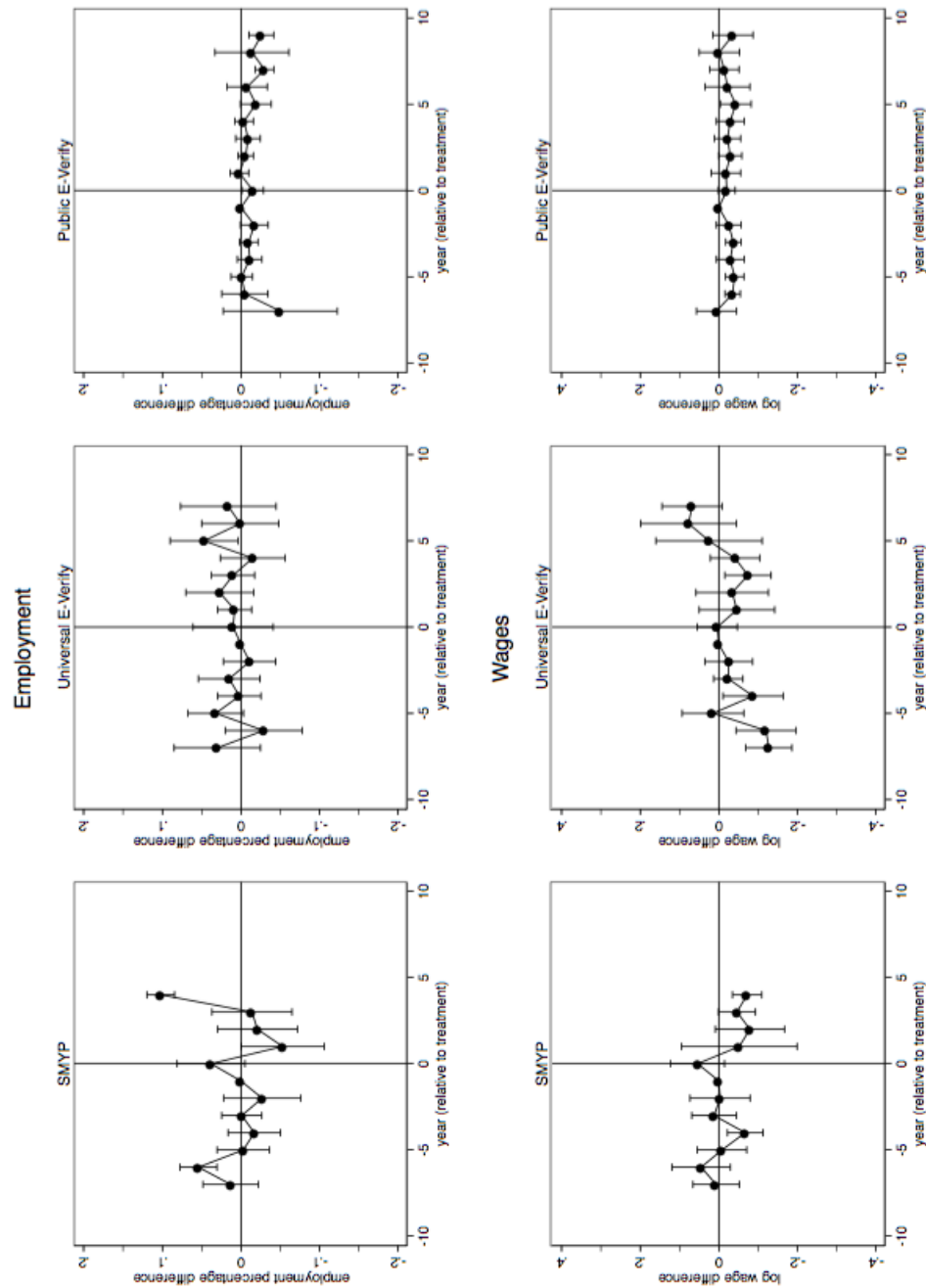
Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The sample is restricted to low-skilled, US-born non-Hispanic men from ages of 16-64. The graphs plot the differences in the estimated effects of SMYP, universal E-Verify mandates, and public E-Verify mandates on employment percentage and log real hourly wages between individuals in the affected states and control states relative to a law implementation year of 0. These estimates net out state and year fixed effects. See section 5.3 for more details.

Figure A.4: Naturalized Hispanic Dynamic Effects (Women)



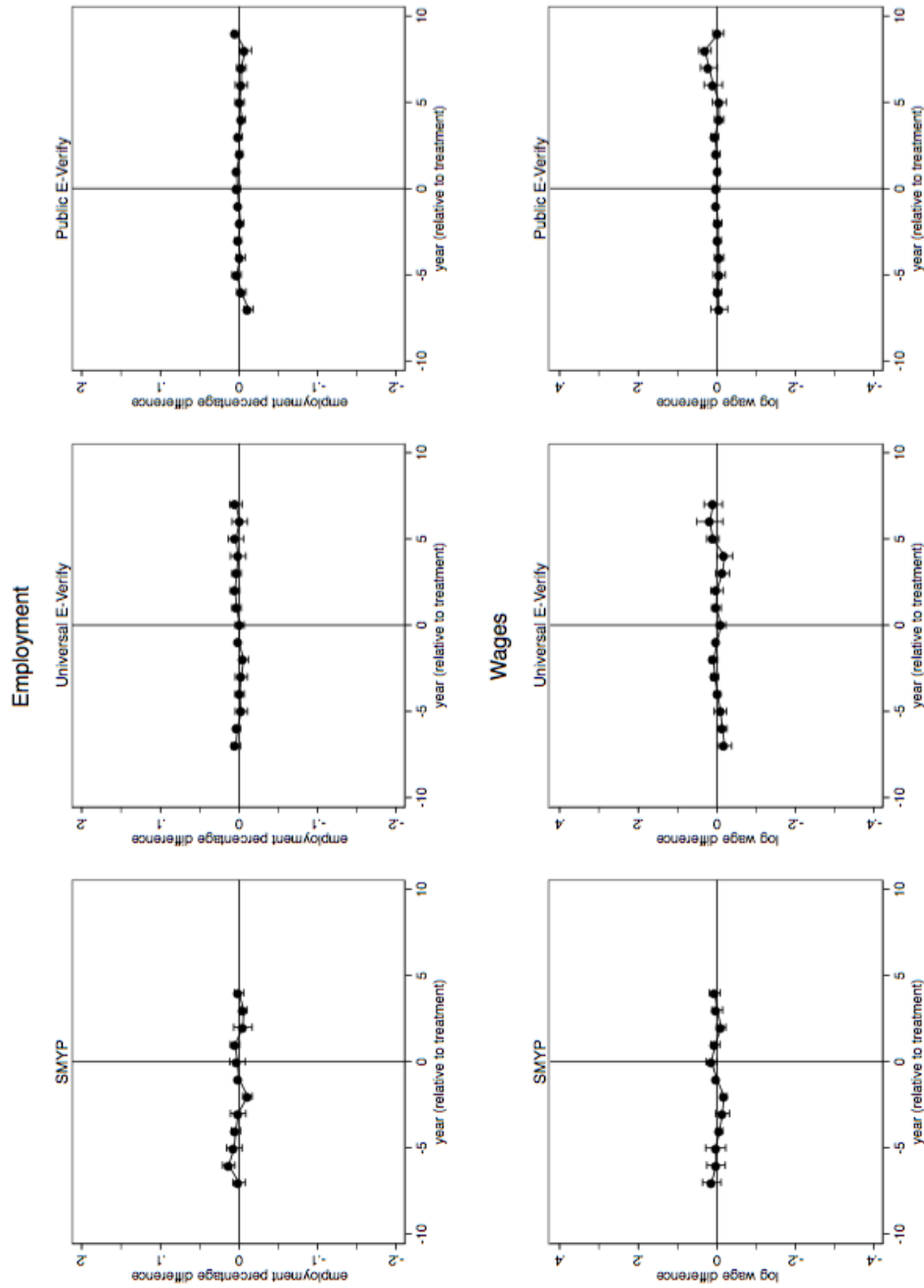
Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The sample is restricted to low-skilled, naturalized Hispanic women from ages of 16-64. The graphs plot the differences in the estimated effects of SMYP, universal E-Verify mandates, and public E-Verify mandates on employment percentage and log real hourly wages between individuals in the affected states and control states relative to a law implementation year of 0. These estimates net out state and year fixed effects. See section 5.3 for more details.

Figure A.5: US-born Hispanic Dynamic Effects (Women)



Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The sample is restricted to low-skilled, US-born Hispanic women from ages of 16-64. The graphs plot the differences in the estimated effects of SMYP, universal E-Verify mandates, and public E-Verify mandates on employment percentage and log real hourly wages between individuals in the affected states and control states relative to a law implementation year of 0. These estimates net out state and year fixed effects. See section 5.3 for more details.

Figure A.6: US-born non-Hispanic Dynamic Effects (Women)



Notes: 2005 - 2015 American Community Survey (ACS) data adjusted to 2015 dollars. The sample is restricted to low-skilled, US-born non-Hispanic women from ages of 16-64. The graphs plot the differences in the estimated effects of SMYP, universal E-Verify mandates, and public E-Verify mandates on employment percentage and log real hourly wages between individuals in the affected states and control states relative to a law implementation year of 0. These estimates net out state and year fixed effects. See section 5.3 for more details.

Appendix B

A Visual Depiction of Tax Saliency Implications

Let p be the price of a good and q be the quantity of a good. Suppose that in the initial free market equilibrium, p_1 is the equilibrium price and q_1 is the equilibrium quantity. Now, let the government impose an ad valorem sales tax of rate τ^s on consumers. This situation is represented graphically in Figure B.1. In the tax distorted equilibrium, the demand curve pivots down and quantity drops to q_2 . Likewise, the price sellers charge rises to p_{2s} and the price they receive falls to p_{2d} . The deadweight loss is represented by the yellow triangle.

Now consider what happens when saliency effects are introduced. Figure B.2 provides a graphical depiction of a tax distorted equilibrium in the presence of consumer under response to tax rates. We see that consumers do not alter their behavior by as much as in the standard neoclassical model. Equilibrium quantity drops from q_1 to $q_3 > q_2$. Similarly, seller prices also do not change by as much. The most notable feature of this model is that the deadweight loss of the tax is potentially much lower in the presence of saliency effects. The new deadweight loss (represented by the green triangle) is smaller than the old deadweight loss. This could have large implications on the efficiency of tax policies as larger tax rates are seemingly more efficient in the presence of saliency effects.

To see the implication of saliency on tax revenue, consider that in Figure B.1, tax revenue is equal to the rectangle formed by the coordinates $(0, p_{2d})$, $(0, p_{2s})$, (q_2, p_{2d}) , and (q_2, p_{2s}) .

Realizing that $p_{2s} = p_{2d}(1 + \tau^s)$, then mathematically, the amount of tax revenue raised is simply $R = q_2 p_{2d} \tau^s$. In the model with tax salience, we can see that the tax distorted equilibrium quantity rises to $q_3 > q_2$ and that the equilibrium price that sellers charge rises to $p_{3d} > p_{2d}$. Notice, though, that the sales tax rate has not changed. Thus, the new tax revenue $R' = q_3 p_{3d} \tau^s$, which is clearly greater than R .

Figure B.1: Deadweight Loss with a Sales Tax on Consumers

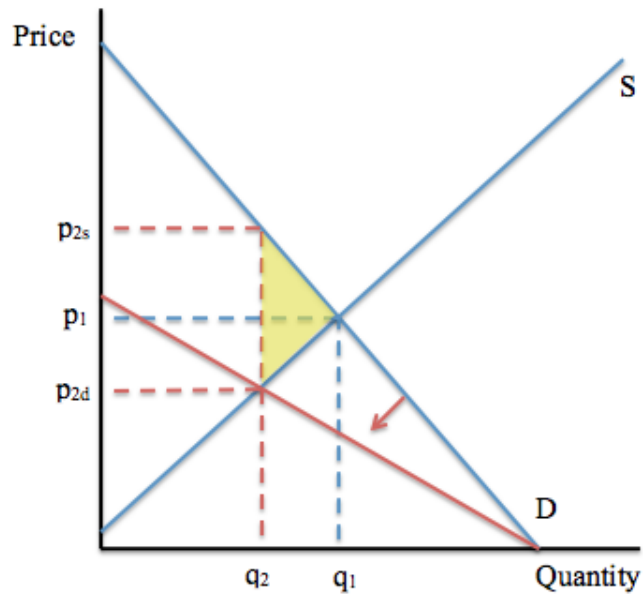
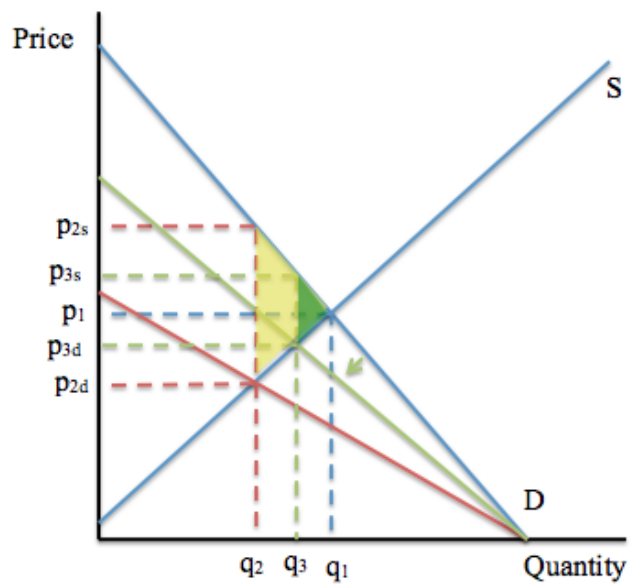


Figure B.2: Deadweight Loss with a Sales Tax on Consumers and Saliency Effects



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